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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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THE

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PHILOSOPHICAL JOURNAL.

Summary of the most important Geognostical Phenomena with which it is necessary to be acquainted in Preliminary Mining Operations. By the late FREDERICK MOHS, Councillor of Mines at Vienna, and Knight of the Royal Saxon Order of Civil Merit, &c.*

IT is quite natural that when we seek for any thing, we must know *what* we are to seek for, and *where* we are to seek for it. I shall afterwards speak of *what* is to be the object of our search. The *where* determines the places in which preliminary mining operations, or the investigations generally, are to be undertaken.

These districts are either whole groups of mountains or portions of them, or they are low tracts of country, distinguished by their external aspect, which lie between two or more mountain groups, and separate these from one another, generally by means of inconsiderable inequalities of surface, which gradually become lower, and finally disappear. They are termed plains.

A mountain group is composed of mountain-chains, which, in a certain order, or by certain combinations, form more or less considerable elevations above the surrounding more level tracts, or above the level of the sea. Its *external* form depends on its *internal* constitution, and is undoubtedly very important to the geognost in a general point of view. It is also not without its value in the search for useful minerals. But the rules and instructions which are derived for this object from the external form of *individual* mountainous districts, or from *individual* mountain ranges, or from the nature of the acclivities of the valleys separating those from one another, &c., possess no universality, and are merely abstracted from a few observations made by those who have given such examples, and are therefore chiefly local, and even in this limited point of view not to be depended on. Whoever will cast a glance over the important mining districts of Joachimsthal, Freiberg, Clausthal, Schemnitz, and Redruth, will be convinced of this, and will find phenomena, at a short distance from one another, which both support and contradict such rules.

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* Geognostical views similar to some of those advanced by the late M. Mohs in this article, and in the other portions of the summary, afterwards to be published, were proposed by us to the Wernerian Society in the year 1813; and accounts of them are to be found in the 2d vol. of the Memoirs of the Wernerian Society, the 1st vol. of the Philosophical Journal, and the early volumes of Thomson's Annals.—EDITOR.

Structure of Granite.—It is of much greater importance, to consider the manner in which various rocky masses or formations are arranged in the mountainous tracts, or what is generally termed the superposition (*Lagerung*) of these mountain-masses. We can generally distinguish a mass which forms a nucleus, or a centre in a geognostical sense, around which, to certain limits, the other rocky masses are arranged and distributed. The centre very frequently consists of a more or less extensive mass of granite, sometimes oblong, and sometimes of a massive form, that is, its horizontal extent is greater in one direction, or it is pretty much the same in all directions. Sometimes this portion of granite is the only one of this mountain-rock which makes its appearance in the combination of rocks forming the tract; but at other times there are several of them, at more or less considerable distances from one another, which occur on a line corresponding more or less to the longitudinal direction of the mountain-group; or they lie in such a manner, that a continuous straight line does not unite them.

Generally these portions of granite occupy the high mountain-ridges, that is, those parts of the mountain group from which, towards both sides (in oblong groups), or towards all sides (in massive mountain-groups), the height diminishes gradually, though very rarely in a uniform ratio, until the base of the mountains is reached. There are, however, cases in which the granite masses present themselves only on one or the other, or on both sides, that is, on the declivities of the mountain groups, while they do not appear at all in the highest regions; or, if they do so, are associated at the same time with other mountain-rocks.

These granite masses often exhibit a remarkable internal structure. They consist either of tabular, or of cubical and cuboidal, or of irregularly-formed masses, so that in the last case they are bounded by an indefinite number of surfaces placed in an entirely indefinite position. These surfaces are by some regarded as rents *(klüften)*, that is, bounding surfaces which have been produced by external agency after the formation and induration of the rock. When, however, we break a piece of granite bounded by such surfaces, and compare the fractured surface thus produced with these surfaces, we find that the two are of an entirely different description, and therefore must have had a different origin; and, when we carefully examine the first, we remark that the individual component parts, that is, the individual parts of which granite consists, in so far as they appear in these so-termed rents, are bounded by surfaces which correspond essentially with those by which the individuals of a twin crystal, or by which granular or columnar distinct concretions of calcareous spar, are separated. What were formerly termed *abgesonderte stücke* (distinct concretions) in mineralogy, are now termed *zusammensetzungsstücke* (literally, composition-pieces), because the massive portions of minerals, twin crystals, &c., are composed of the same; and the surfaces which bound these are termed surfaces of distinct concretions *(zusammensetzungsflächen)*.* Besides granite, excellent examples of this structure are afforded by syenite, porphyry, basalt, greywacke (especially the large granular varieties), &c. The indeterminately angular pieces of which a mass of granite is composed, are bounded by surfaces which consist of the distinct-concretion-surfaces of the individual component parts which meet there; and as such surfaces (as we are taught by the consideration of compound minerals, twin crystals, &c.), can only be produced when the individuals or component parts of the one compound mass come in contact with those of the other, *in and during their original formation*, so it follows that these angular pieces of granite, throughout its whole mass, and with them the surfaces pointed out, have not only been produced *at the same time*, but also, that the bounding surfaces or rents, as they have been termed, are not true rents *(zerspaltungen)*, but surfaces of distinct concretions, of a higher order, however, than those which bound the individual concretions of which a rock is composed; and that these angular pieces are actually distinct concretions. *Compound* mountain-masses likewise, that is, those consisting of dissimilar substances, such as granite and limestone, porphyry and gneiss, basalt and chalk, &c., are often bounded by distinct-concretion-surfaces, which belong to a still higher order. From these considerations the following deduction has been drawn, and it is one which is of great consequence for the whole of geognosy: that when two similar or dissimilar mountain-masses, or any other kind of simple or compound mineral substances, come into contact with one another, in such a manner, that they are only separated from one another by surfaces, regarding which it can be shewn that they possess the characters of surfaces of distinct concretions, these rocks or minerals must be of contemporaneous origin;* and this deduction, containing one of the most important criteria of contemporaneity of formation, is of the most extensive utility, more particularly for the correct determination of the relations of the beds of rocks, an acquaintance with which is the only guide that we can follow, without fear of getting into a wrong path, when we wish to investigate mountains, or to search in them for useful minerals.

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* As we are not in possession of such refinements in our geognostical nomenclature, we must throughout this article, as we did in a former one, vol. 28, p. 334, continue to employ the expression distinct concretions for *zusammensetzungs-stücke*, and surfaces of distinct concretion for *zusammensetzungs-flächen*.—EDIT.

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* We do not understand by contemporaneous origin, an origin at a fixed time, but one during an *uninterrupted* time, in which no pause occurred in the formation, of whatever longer or shorter duration it may have been, and which admits of farther determination. Hence, two rocky masses can have been produced at once, or the one can have been formed at an earlier or later period than the other; if their periods of formation are not separated by a sensible interval, they are considered by us as contemporaneous.

To this investigation likewise, and therefore to the attainment of the object which has caused the present treatise, belongs the accurate knowledge of what has generally been termed stratification *(schichtung)*. Of the surfaces which bound the cubic or cuboidal masses of granite, one, and generally that one whose position deviates least from horizontality, is considered as what is termed a *seam of stratification (schichtungskluft)*, and the tabular masses contained between two of such seams are considered as actual strata; while the others, which are nearly perpendicular to them, are regarded as mere rents *(zerspaltungen)*. The first of these conclusions is just as inaccurate as the last; for, when we compare with one another the three different surfaces of separation which are distinguished by their position and direction, and when we compare them with those which bound the indeterminately angular pieces, we find that they correspond with one another perfectly in all essential properties or relations. But we cannot regard any of the surfaces bounding the angular distinct concretions as seams of stratification, as there are no strata present which they could bound. And just as little can the one or the other of the surfaces enclosing the cuboidal masses be a seam of stratification, as they are not only nothing else than that of which we have just spoken, but because they also all possess equal claims to be considered a seam of stratification, while only one of them can be so. Not one of them, therefore, is really a seam of stratification. We certainly find that some varieties of granite are somewhat more easily split in one direction than in another. We might, therefore, be induced to regard as seams of stratification those separating surfaces, which correspond to this direction, and which are thus sometimes distinguished from the others. But this appearance is a mere feature of the stony structure, and occurs only in those varieties of granite in which the first commencement of a transition into gneiss is noticeable in the somewhat lengthening form of the portions of mica and felspar. Where this does not take place, the surfaces of separation, or the distinct-concretion-surfaces, are all alike, and hence, not one of them can be regarded as a seam of stratification. This surface of separation, however, disappears entirely in gneiss itself, while the others are distinctly seen.*

It is now easy to make the application to the tabular masses into which granite appears to be sometimes divided. The surfaces which bound these, have precisely the characters of the surfaces of distinct concretions, and the divisions considered as strata, are here nothing else but tabular distinct concretions. The appearance can be explained without difficulty. We frequently find, particularly in masses of limestone, that a twofold composition *(zusammensetzung)* or structure is present, from which tabular distinct concretions can be produced, and that sometimes the one, sometimes the other, is more developed and becomes more apparent, or that it becomes more and more indistinct, and disappears; whence proceed many, at first sight, apparently remarkable phenomena. This may also be supposed in granite, in order to understand easily in what connection the tabular distinct concretions stand with the massive, and in order to be convinced that the one proceeds from actual stratification just as little as the other.

The granite masses, in whatever form they appear, are generally surrounded by thick masses of slaty rocks. The cases in which other features present themselves, may here be passed over. The slaty rocks exhibit the extremely remarkable phenomenon, that, in respect to their structure, and the relations of their beds on the great scale, they present the same features as if granite-masses were *not present*. It was believed that the slate rocks surround the granite masses in a mantle-shaped manner, and that the latter had exercised such an influence on the first, as to determine the form of arrangement of their beds. But further investigations have proved that this occurrence so rarely takes place, that, where it has actually been found by *accurate* observation to occur, it may be merely regarded as an exception; and that, on the contrary, in the usual cases, the masses of the slate rocks are just as little disturbed by the granite as if that rock differed from the slaty mass only in structure.

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* Thus gneiss, and most of the slaty and other mountain masses are not stratified, but the tabular divisions observable in them are referable to structure, and are mere distinct concretions. The criteria for distinguishing real from apparent stratification, derivable from the ideas of the formation of this rock, cannot be fully discussed in this place. What has been already said is sufficient to shew, how necessary it is to take care not to regard all as stratification which possesses its external aspect.

Structure of Slate-Rocks.—The various relations exhibited in the arrangement of the beds of the slate-rocks, are the most important which can be ascertained, in order to aid the search for useful minerals; for these are the rocks which chiefly contain such repositories.

These relations, when viewed on the large scale, consist more especially in the following:—That the various masses, which constitute the slate rocks, generally preserve throughout the whole mountainous tract a uniform direction (a similar strike); and that the deviations from this direction, which are frequently met with, and which often amount to a considerable number of degrees, mutually neutralise one another, as they are just as much on the one side as on the other. This remarkable phenomenon sometimes even exists on a larger scale. It is frequently observed, that in neighbouring mountain-groups, that is, such as are separated from one another by less elevated tracts (plains in the sense mentioned above), the general direction is identical or similar; and most mountain-groups are thus united into a whole, or as it were into a unity, which is the largest that is reached by geognosy in considering the constitution of mountain-masses. But we must beware that we do not extend this process of generalization beyond its limits, at least that we do not extend it over the whole earth, that is by assuming that this direction is the same round the whole earth.

Classes of Rocks.—The slate-rocks consist of a considerable number of mountain-masses, which are regarded by geognosts as belonging to different formations, and separated by them into two classes, of which the one is termed Primitive and the other Transition. The mere name of the latter class, which presupposes a third, and which, as derived from the nature of the mountain-rocks and of the mountain-masses, is quite correct as a mere name, leaves it to be understood how it stands in respect to the division of classes. When we have two unities (two species or genera in the mineral or vegetable kingdom, or two classes in the geognostical systems), which are combined by means of a third unity of the same degree of extent, there can exist between these three unities no sharp boundaries; and nature confirms the idea in the case of the different classes of mountain-masses, that there are really no sharp boundaries between them. The division, therefore, has no sufficient foundation, and therefore no value. It is necessary, in the search for useful minerals, to have this well in view, in order that we may not lay too much weight on the division of mountain-masses, and especially on the classes of the primitive and transition rocks, inasmuch as we might thus be restrained from making a search for any thing in a mountain-mass of one division, regarding which he believes that it is only to be found in one belonging to the other.

On Formations.—With respect to the subdivision of the classes, or of the mountain-masses generally, into formations, whose limits are chiefly founded on a certain sequence of superposition, it is not much better, especially in the primitive and transitive classes, for this sequence (which is also termed the series in regard to *relative antiquity* of the mountain-masses), in respect to the masses which are distinguished, is not only *not constant*, and on the contrary subjected to a diversified change, but *the mountain-masses of these different formations pass into one another;* as all geognosts know and admit, without sufficiently attending to the *consequences.* It is always dangerous, and quite contrary to a scientific mode of proceeding, to employ a notion of whose correctness and precision we are not convinced; and it is so much the more unsafe to do this in an occupation which is so difficult, and which requires so much attention, as the searching after useful minerals, especially when we previously know the untenable nature of such a notion. Nevertheless, *the idea* of formations, although it cannot succeed in determining the individual formations with the requisite certainty and sharpness, is not only very useful for those who devote themselves to this occupation, but also very important as a valuable guide; and I shall, in another place, speak at greater length of this utility and importance.

The mountain-masses which constitute the slate rocks are more particularly gneiss, mica-slate, clay-slate, with its associated talc and chlorite slates, greywacke-slate, if there is sufficient reason to separate it from clay-slate, and greywacke, which is directly connected with the greywacke-slate.

Junctions of Rocks.—We generally find the gneiss next the granite; but this is not without many exceptions, of which we have remarkable examples in the Hartz, where greywacke-slate is in direct contact with granite, and in Cornwall, where the killas, a rock intermediate between mica-slate and clay-slate, occupies that position. Where gneiss and granite bound one another, these rocks, which moreover are merely different in structure, often pass directly into one another; more rarely they are separated from each other by true distinct-concretion-surfaces. The stony structure of the gneiss generally changes in the vicinity of the granite, and is bent for a short distance, because it as it were presses closely on the granite, whence observers have frequently deduced the existence of a mantle-shaped arrangement of the beds; and even the structure of the granite itself not unfrequently undergoes a change, inasmuch as it becomes sometimes more coarsely granular (as in what are termed *Stockscheidern*), and sometimes more finely granular (as we find in the granite masses of *Stockwerks* in gneiss), but generally it presents a tendency to the slaty structure. However, there are likewise cases in which none of all these phenomena exist, and where both rocks, with their prevailing characters, are sharply joined and firmly grown together.

The structure of the mountain-masses of gneiss, under which is here understood what is generally termed stratification in this and the other slaty rocks, is frequently cut by the granite, or, as it is also expressed, the strata of gneiss abut against the granite; and this latter is far more correct, for cutting through is merely a figurative idea, which includes more than can be observed. Here, therefore, we have not the phenomenon of a *conformability*, but that which has been termed *unconformability* of the beds; but the *consequences* do not follow which are generally drawn from a deviating disposition of beds:* viz. that granite and gneiss are not of simultaneous origin, and even that they are dissimilar in nature; for the forms[*] in which the former appears in the latter, and the above-mentioned transitions, incontrovertibly prove the contrary.

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* These consequences could only take place if the tabular structure of gneiss and the other slaty rocks were real stratification.

In the vicinity of the mutual boundary sometimes the one, sometimes the other, sometimes both of the above-mentioned mountain-rocks, contain minerals which do not belong to their component parts, such as garnet, tourmaline, and hornblende; the last more especially, when the granite is syenitic, or is actually a syenite, a rock which is almost too closely connected with granite to entitle it to be distinguished by a separate name. Connected with this occurrence, but of more importance for the search after useful minerals, is the fact that, at the junction of the rocks, repositories of these minerals not unfrequently occur, which do not belong to the composition of the general mass. Such junctions, therefore, must be carefully attended to, and where there is an opportunity, must be minutely examined.

When mica-slate is in immediate contact with granite, the phenomena are essentially almost the same as we have described above. Only the granite is not unfrequently very rich in quartz, and the mica-slate often acquires felspar near the junction, and is thus converted into gneiss. Clay-slate often exhibits a greater diversity of aspects near the granite. It sometimes assumes the characters of gneiss or hornblende; and what is called greywacke-slate becomes converted into a rock which is called *hornfels* in the Hartz, which passes directly into the greywacke-slate, and is yet connected with the granite, inasmuch as it contains some felspar. This rock, moreover, occurs at a greater distance from the granite in greywacke rocks, and hence is not connected with the immediate vicinity of greywacke rocks to granite.

No determinate arrangement of the Primitive and Transition Rocks.—The individual mountain-rocks of the slate series usually alternate with one another in the form of beds; but there is no fixed sequence in this alternation. Sometimes the mica-slate lies on the gneiss, then the clay-slate on the mica-slate, and finally the greywacke on the clay-slate. From single observations of this kind, a general rule has been laid down. But the exceptions from this rule are just as numerous, if they are not more numerous, than the cases conformable to it. In order, however, to find a conformability to rule in this irregular alternation, and in order to deduce from it determinable formations, not only have rocks, which, in another series, would have been termed gneiss and clay-slate, been denominated greywacke or greywacke-slate, but it has been assumed that to granite there succeeds first an alternation of granite gneiss, then pure gneiss; to it an alternation of gneiss and mica-slate, then pure mica-slate, &c. But none of all the possible combinations of these, and, if regard were paid to the series of beds included in them, none of the double or more compound combinations of included beds amongst themselves or with others, should be passed over, if the diversity of nature were to be represented; and it would then result from these that this diversity includes *all possible* sequences, and that therefore a certain *determinate* order, and one which is sought as a consequence of a theory, cannot exist.

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[*] Some of these forms are among the characters by which contemporaneous formation is directly recognised.

Hardly any one can misapprehend the importance, in reference to the search after, and discovery of, repositories of useful minerals, of the knowledge, that a fixed order in the superposition of the mountain-rocks, from which a fixed sequence had been deduced, does not exist in what are termed the primitive and transition series. For when, for example, we know that in a certain district, the beds or veins, of which we are in pursuit, only occur in mica-slate, and not in the gneiss and clay-slate, and we find the gneiss on the lying side, and the clay-slate on the hanging side, of the rocky mass in which we have hitherto instituted our investigations, we could not hope for a fortunate result either beyond the former or the latter, if we should adopt as correct the assumption of the invariable sequence of the rocks, and would hence perhaps consider as superfluous the further investigation of the neighbourhood. But mica-slate can make its appearance under the gneiss and above the clay-slate. So long as we remain *within* the boundaries of the slaty rocks, we may expect generally to meet with every succession of the individual members, and must acquire a knowledge by *direct* observation, of the phenomena of a particular mountain-district or a particular portion of it, without allowing ourselves to be guided by any theory or by any system; and this knowledge is, as the sequel will shew, of the utmost importance.

Transitions of Rocks.—The rocks composing the individual members of the slate series pass immediately into one another. The transitions of the gneiss into mica-slate, and of the mica-slate into clay-slate (including greywacke-slate) are generally known. But these rocks likewise pass into the common or granular greywacke, and this transition deserves to be more attentively considered. The gneiss first of all has its structure altered, and becomes granular, without acquiring any resemblance to granite (for there are other varieties of gneiss which form the passage into granite), the mica loses its usual characters, more especially the perfection of its cleavage, and begins to resemble clay-slate, and the felspar and quartz appear of a grey colour passing into green, which is generally the colour of the whole mass. The structure is at first angulo-granular, but not without occasionally passing back into the slaty; and the rock, when the size of the concretions is diminished, bears so great a resemblance to *some sandstones* that we are hardly able to discriminate between them. On the other hand, the concretions or grains lose less in size than in the sharpness of their edges; the mica becomes closely united to them, and as its quantity and that of the felspar gradually diminish, rocks are formed, which it would not appear strange to us to meet with in the most characteristic greywacke district. This transition, which occurs very frequently under all modifications, only admits of a general account here; and we need not further explain in what consists the application of its knowledge in aid of the search for useful minerals.

But, nevertheless, another circumstance, connected with these transitions, deserves some observations. It is generally the case, that the mountain-masses pass into one another just as they lie on and over one another; and in such a manner, that when on a bed of gneiss there reposes a bed of mica-slate, and on the latter a bed of clay-slate, the first passes into the second, and the second into the third. We may term this the transition *in the direction of the thickness.* But the occurrence is often met with of transitions *in the direction of the strike and of the dip.* When we are occupied with a bed of gneiss, and advance in the direction of its strike, we not unfrequently find that the rock is gradually changed into mica-slate, into clay or hornblende-slate, or even into a (sometimes red) sandstone. Transitions of the last-mentioned kind are, however, more frequent in the direction of the thickness. Since we know how different are the features of repositories of useful minerals in different mountain-masses (although this is certainly not without exception), we can easily understand with what care we should observe the transitions in the direction of the strike and dip; and how little confidence should be placed in *single* sections, which have been prepared to assist the investigation of a district, although they have been sketched with all possible exactness, and which, taken by themselves, are therefore quite accurate; and hence, that it is indispensably necessary to become possessed of *as many* of these sections as possible. Thus, no lateral valley should remain unexamined in reference to such transitions; and if the mountain ridges between them possess too considerable a breadth, they also should be sufficiently investigated, so that all means may come into operation which serve, or at least can give assistance, in accomplishing a task of so uncertain an issue, with that degree of precision which is necessary, if a decisive result is to be expected. * * * * * * *

Rocks subordinate to the Slate Series.—The slate series, which we have hitherto been considering according to the relations that are presented in reference to the discovery of useful minerals, is, as it were, the general repository of several other mountain-rocks, which appear imbedded in it; and a knowledge of which, as well as of the phenomena of the arrangement of their beds, is just as indispensable as that of the slate series itself. Some of these agree so perfectly with the slate rocks, from which they are separated by some observers, and are so directly connected with them, that, in a purely geognostical point of view, they need hardly be enumerated. These are the masses of talc-slate and chlorite-slate, which generally occur in great variety in the slate series. In regard to the search for useful minerals, the distinction of these mountain-masses is not *a matter of such indifference ;* for some of the particular repositories, namely some of those which are of a bed-like nature or constitution, seem not only to be more frequently contained in them than in the other slaty rocks, but appear to be as it were peculiar to them. But it must be remarked that this circumstance is a very general one, and that, therefore, it becomes necessary for us to study, particularly in this respect, a district which is to be examined, and not merely to transfer the result obtained in other districts, even of one and the same mountain-group ; because one and the same zone or region, which *here* consists of such mountain-masses, or at least contains such mountain-masses in considerable quantity, *there* consists of others, or contains little or none of them. There is often occasion to make the observation, that a bed of this description (likewise of many other kinds of rocks, which cannot here be specially detailed), without possessing considerable thickness, or any other distinguishing features, is continued for very considerable distances, disappears here and there, or becomes so little noticeable, that we can scarcely recognise it ; that afterwards, however, it makes its appearance in its full extent, and that it, perhaps, undergoes this change several times throughout the extent of its prolongation. Although, also, there may be nothing to seek for in the bed, yet not unfrequently it affords us indications which will be valued by those who are engaged in pursuits of this description.

Another portion of the imbedded mountain-masses differs more in its nature from the slate-rocks than those of which we have hitherto spoken. I allude more especially to granite, porphyry, the trap rocks,* limestone, and some other rocks of inferior consequence, such as serpentine, quartz, &c. Granite appears frequently at the boundaries of the central masses in individual beds in the slate series, of whatever nature the latter may be, and its phenomena are generally the same as those of the beds which we have already considered; but it also occurs at a greater distance (geognostical, not geographical distance reckoned by miles), and, in this case, it is found more rarely in regular beds than in irregular masses of all sizes, of which the larger, in reference to the above relations, are to be duly attended to.

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* These rocks, on account of a particular form of arrangement, in which they frequently present themselves, have been regarded as of a peculiar origin, inasmuch as they are supposed to have issued from the interior of the earth as burning liquid or molten masses. Limestone sometimes exhibits the same geognostical relations. But it is not contrary to theory to attribute the same origin to it,—an origin which, indeed, can perhaps only be denied to a formation which has evidently been formed in a mechanical way. The large granular greywacke, consisting of pebbles as large

Connected with the granite there are two other mountain-rocks, which, it is true, do not occur everywhere, but when they are met with, are not unfrequently worthy of notice. The first of these is porphyry, the other syenite. We might also include here the greenstones (dolerite and diorite), which, in all their relations, resemble syenite, porphyry, and granite; but they appear to stand in somewhat nearer connection with other mountain-masses, and hence can be more properly enumerated with them. What are termed trachytes are partly to be considered as porphyry, partly as granite, and partly as a sort of greenstone, for their determination as a peculiar kind of mountain-rock is founded on a purely theoretical assumption, which is proved by no direct observation, and can still less be recognised from the nature of the rock. "That kind of porphyry is trachyte which is of Plutonic origin."*

Porphyry is nothing but a porphyritic granite, whose basis consists of so intimate and fine a compound, that the indivi-

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as hazel nuts, united together by a cement of very quartzose clay-slate, is, without doubt, evidently a mechanical rock, at least it is generally regarded as such. But it has been observed, that irregular masses of it, like the masses of granite, porphyry, and trap, penetrate the slate-rock (a thin, slaty greywacke-slate), distinctly and decidedly cut through its structure, and, although sharply separated from it, yet closely adhere to it. They do not throw the structure of the slaty rock into the smallest disorder, just like the other rocks mentioned above; and they present the same phenomena as these. Why should a common origin not be assigned to all these mountain masses? As to whether this common origin was Plutonian or Neptunian, or if it can have been one of the two, is another question.

* Geology and Mineralogy considered with reference to Theology, by the Rev. William Buckland.

the slate series. In regard to the search for useful minerals, the distinction of these mountain-masses is not *a matter of such indifference*; for some of the particular repositories, namely some of those which are of a bed-like nature or constitution, seem not only to be more frequently contained in them than in the other slaty rocks, but appear to be as it were peculiar to them. But it must be remarked that this circumstance is a very general one, and that, therefore, it becomes necessary for us to study, particularly in this respect, a district which is to be examined, and not merely to transfer the result obtained in other districts, even of one and the same mountain-group; because one and the same zone or region, which *here* consists of such mountain-masses, or at least contains such mountain-masses in considerable quantity, *there* consists of others, or contains little or none of them. There is often occasion to make the observation, that a bed of this description (likewise of many other kinds of rocks, which cannot here be specially detailed), without possessing considerable thickness, or any other distinguishing features, is continued for very considerable distances, disappears here and there, or becomes so little noticeable, that we can scarcely recognise it; that afterwards, however, it makes its appearance in its full extent, and that it, perhaps, undergoes this change several times throughout the extent of its prolongation. Although, also, there may be nothing to seek for in the bed, yet not unfrequently it affords us indications which will be valued by those who are engaged in pursuits of this description.

Another portion of the imbedded mountain-masses differs more in its nature from the slate-rocks than those of which we have hitherto spoken. I allude more especially to granite, porphyry, the trap rocks,* limestone, and some dual component parts can no longer be distinguished from one another. The transitions to be observed prove this. As the granites, in reference to the relative quantities of their constituent parts, are so extremely different, porphyries with different kinds of basis are thence produced, which are distinguished as hornstone, felspar, and claystone porphyries. Occasionally, portions of granite, syenite, or gneiss, arise in the general porphyry-mass, partly as irregular masses, partly as apparent fragments, thus affording a direct proof of the origin of the porphyry. Porphyries appear as beds, as veins, and also as irregular masses in slate-rocks ; and the last are often of such extent, that not only do slate and other mountain masses arise from among these porphyries, also in more or less regular bed-like forms, but they supplant the latter for considerable distances. The beds and veins of these mountain-masses do not appear to be of much importance in reference to the discovery of useful minerals ; but their large masses, on the contrary, are worthy of particular attention, because, as we know from experience, in many districts they are very rich in ores. At their meeting with the slate series, they present the same phenomena as granite does, inasmuch as both the rocks in contact are partly reciprocally altered, partly pass directly into each other, just likewise as similar passages take place between granite and porphyry, and these sometimes are so gradual, that we must pass through a great number of varieties before we find the transition completed. Very frequently, extensive masses of porphyry replace the whole, or, at least, a portion of the slate rocks. The consequence of this is, that generally on one declivity of a mountain-group there are no porphyries whatever, and we meet only with slaty rocks ; whereas on the other side, large tracts of porphyry present themselves. The porphyries also pass into sandstones, viz. the red sandstones, and into conglomerates, whose cement is often very much like sandstone, and which alternate many times in regular beds with the most characteristic porphyries. Phenomena of this description may undoubtedly create doubt as to the generally received opinion of the nature of such conglomerates, I mean the opinion that they consist of pebbles derived from previously existing mountain-masses ; but such discussions are not appropriate in this essay, which is not so much intended for the correction of geognostical knowledge, as for the application of that knowledge to a particular object.

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Syenite occurs more rarely in well characterized beds and veins than in irregular masses, but then the relations it exhibits are almost the same as those of porphyry, associated with which it is often found, and into which it passes, as it does into granite. Regular beds of this rock, and also well characterized veins, which, as has been already remarked, not unfrequently occur, are not of special consequence. Extensive irregular masses, however, are to be carefully noticed as in the case of porphyry.

What are usually called traps consist chiefly of two rocks, which, in respect to their component parts, correspond at least so far, that they contain hornblende and, more rarely, augite in preponderating quantity, which determines the colour of the rock. These two are greenstone and hornblende-slate. Some others, as amygdaloid, wacke, greystone, porphyry-slate, basalt, &c., of which several are generally included under the trachytes, may here be passed over, as they rarely contain repositories of useful minerals. The greenstone, for the most part, partakes of the features described as belonging to the rocky masses, which occur imbedded inthe slate series; it is found in veins and beds, but especially in extensive irregular masses; it passes into porphyry, into syenite, into hornblende-slate, and also into gneiss; and sometimes it abounds in ores. Hornblende-slate itself agrees with the slate rocks in its geognostical relations, and what has been said here of them applies likewise to it. It passes into gneiss, into mica-slate, and into clay-slate, in the direction of its thickness, as well as of its strike and dip, and the transitions are so similar to those described above, that it would be superfluous to dwell longer upon them. In some districts, both hornblende-slate and greenstone have proved rich in metalliferous contents, and they ought, therefore, on no account to be passed over unnoticed.

Limestone is a mountain rock of especial importance. It occurs partly in the central granitic mass, especially when the latter possesses a tabular structure, or, according to the usual expression, when it is stratified. It is found in a similar way in the various slaty mountain masses, and there presents very remarkable relations. It has been supposed, that the limestones lying next the central mass (what are termed primitive limestones) possess the most distinct, and frequently a large granular structure; those more remote (what are termed transition limestones) a less distinct structure, at least one which is less coarsely granular; and the secondary limestones, to speak generally, more of an earthy composition; and there are undoubtedly very many examples which countenance such an opinion. But nevertheless, the phenomenon is not one of universal occurrence, and, even in masses which are connected together, all these differences of structure present themselves next one another. It would, therefore, be a fallacious mode of proceeding, to take the size of the grains of the limestone as the test of its geognostical distance from the central mass, a distance which can only be determined by superposition.

Limestone occurs partly in pretty regular beds, partly in irregular masses, both of which have very various dimensions,—the first being sometimes of almost inconceivable extent. They seem to increase in thickness and extent as their distance increases from the central masses, or, where such do not exist, from the central chain; and they, as it were, acquire one of the chief characteristics of the slate-rocks, inasmuch as they generally form on both sides of the central chain, a peculiarly long and high chain, in which again many mountain masses are contained in the same, and not unfrequently, very remarkable manner, as the slate series includes porphyry, syenite, and some other mountain rocks.

Beds and Veins.—The beds of granite, porphyry, syenite, greenstone, limestone, &c., which occur in the slate-rocks, and which are generally conformable to their structure on the small and on the large scale, nevertheless very frequently cut this structure at some places, thus proving directly that it is not stratification; for true stratification can be cut by no bed, which itself must be regarded as a stratum possessing particular characters,—as directly follows from the idea of stratification. It follows from this phenomenon, that beds of this kind must very often be irregular, especially as to their thickness, and observation confirms this, inasmuch as it teaches us, that such beds sometimes increase in their dimensions, sometimes become contracted, and sometimes are interrupted for more or less considerable distances. The slate series of rocks does not generally undergo thereby any change in the direction of its structure,—at least any important change, and the change it may present is only near these beds. If the increase of thickness of such beds be considerable, and if an interruption immediately ensue, the portions assume the form of irregular masses, or of what are termed, lying irregular masses (*liegende stöcke*), and it not unfrequently happens, that the structure of the slaty rocks abuts against these irregular masses: that is, they are cut off by them. This happens sometimes by means of a transition, somewhat in the manner above described; sometimes, however, it happens in such a way, that the different mountain masses are sharply united together, or are separated from each by a distinct-concretion-surface. In the first case, the rocky mass sends forth vein-like branches of granular rock into the slaty mass,—such as are known more especially in respect to granite, porphyry, greenstone, &c., and it is not unfrequent to find separate portions of the slate in the mountain mass in contact with it, and likewise separate portions of it in the slate. In the other, the phenomenon acquires the aspect of a shift, and many examples of this kind have been explained by shifts. But when we examine narrowly the distinct concretion-surface, and sufficiently attend to its nature in respect to its general aspect (viz. that it consists of the distinct concretion-surfaces of theseparate individual concretions composing the rocks, and not of fracture or superficial surfaces); when we attend likewise to its streaking (where such exists), to its position, and to the bendings which it generally makes, and which, in all the other hitherto mentioned relations, are not compatible with the notion of a displacement of the rocks in regard to each other; we are easily convinced, that both masses have been originally formed with the same relations, and in the same position as at present,* and this conviction is of great moment for the search after repositories of useful minerals, because all the other phenomena of superposition stand in exact correspondence with this original formation, and we shall therefore not thus be misled to regard this appearance in an erroneous point of view. It is known that there are phenomena of this kind, which seem to prove incontrovertibly a displacement of the rocky masses. But when we follow up such appearances in their full connexions, we meet with such as present the same phenomena, and yet directly make known to us that an actual displacement could not have taken place. In order not to become discursive on this subject, whose further elucidation does not belong to the present subject, we must content ourselves with the remark, that we not unfrequently find apparent displacement at such points where limestone and granite are directly in contact. We see that these mountain masses are partly bounded by the most characteristic distinct-concretion-surfaces (and these are, when we do not consider the nature of these surfaces, the most deceptive appearances), partly, intimately, and strongly united with one another, and combined in such a manner as to render it impossible accurately to assign boundaries to the two; and, finally, we see them so dovetailed into one another, and as it were fastened together, that every thought of a movement having taken place must disappear.

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* See ante, p. 4.

The tabular structure of the limestone next the contact with the granite, is generally, where present, in the greatest regularity, and the changes which the two mountain-masses produce, are mutual. But what completely overturns the opinion that phenomena of this kind are produced by the actual movement or displacement of the parts of the mountain-mass, are the observations which have been made on veins which seem to be displaced, while the portions of the mountain-mass in which their different parts occur have preserved their original position. We ascertain this from the fact that bed-like masses in the mountain-mass *through which* the displaced veins or fissures, or, in one word, *the separating surfaces proceed*, and which must accordingly have also been necessarily displaced, have suffered no shift. Since, then, phenomena of this kind cannot be explained by shifts, while those mentioned above (together with these) can be explained in a manner more consonant with the phenomena of nature, we must, in reason, consider well before we admit actual movements or displacements of a portion of the mountain-mass at a bounding-surface, whether that be a mere distinct-concretion-surface, a rent, or even a distinct vein, although appearances may be favourable to a belief in such a shift.

It is evident from the above, that mountain-masses of inconsiderable dimensions, which, generally speaking, lie conformably to the structure of the slaty rocks, and hence are considered as bed-like, and are termed *beds*, yet nevertheless cut this structure here and there, sometimes for shorter, sometimes for longer distances; sometimes under greater, sometimes under smaller angles. When the places, in a series of beds, where this occurs, are several, and the cutting angles are greater than those at which the mass lies conformably to the beds, it assumes a vein-like character, and, in one word, becomes a *vein*. *Vice versâ*, a vein is defined to be a bed- (*i. e.* table-) shaped mass, which cuts the structure of the mountain-masses of slaty rocks, for, in others than the slaty rocks, the cutting through cannot be observed. But there are many otherwise extremely characteristic veins, which partly do not do this; and even some which, throughout their whole extent, lie generally conformably to the slaty structure. Under these circumstances, it is difficult, when the beds and veins contain merely rocks like granite, porphyry, greenstone, basalt, &c. to assign the limits between the bed and vein; and, without entirely throwing away the distinction, the consequence has been drawn, and it is a useful one, even for the present investigation, that we ought not to attribute too much importance to this distinction. In regard to the beds and veins which have hitherto been considered, this is of less consequence. It may serve, however, to direct attention to the essential difference between *beds and veins of ore*; and, should it thence perhaps appear that it likewise is not so great as it is generally assumed to be, this may certainly acquire some influence on the procedure which is to be followed in searching for such repositories.

(To be continued in our next number.)

On the Danish Oyster Banks. By M. Henrik Kröyer.

The active editor of the *Naturhistorisk Tidskrift*, whom we have to thank for so many valuable contributions to the Fauna of Denmark, gives us a full account of the condition of the Danish oyster-banks, in a little work published at Copenhagen, entitled *De danske Osterbanker, et Bidrag til Kundskab om Danmarks Fiskerier*. While his treatise is more especially interesting as discussing a subject of political economy, and must have possessed double interest for Denmark just at the moment of its appearance, because the period for farming out the royal banks, as they were considered, had partly elapsed, and was partly impending, and because an opinion had been expressed in Jütland in favour of throwing open the oyster fishery; yet, at the same time, it is not without importance for the natural history of the oyster. It gives us a clear idea of the mode of occurrence of this species of shell-fish; it weakens and overturns many hitherto received prejudices respecting their mode of living; and, even in its statistical and historical portion, is calculated to afford the naturalist much information and amusement.

In the *first* section of his work, the author treats of the natural history of the oyster; and, in the *second*, of the Danish oyster banks—of the mode of taking the oysters, and the requisite apparatus—of the oyster traffic in Schleswig and Jütland—and of the oyster dams. The *third* part contains historical notices of the oyster-banks in Schleswig and Jütland. An engraving represents the various fishing implements; and a particularly interesting illustration of the essay is a map of Denmark and its ducal possessions, in which the banks that are at present fished, as well as those which have been abandoned, are represented. As M. Kröyer not only visited the various oyster localities, and collected many notices on the spot, but has likewise made use of the register and minutes of the Board of Management of the Rents, the results of his investigations are undoubtedly as accurate and exact as can be required in a statistical inquiry of this kind, although certainly they open up no very agreeable prospect for the revenue or for the lovers of oysters. But let us now direct our attention to the separate portions of the treatise, in so far as they are suited to our objects.

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* From Wiegmann's *Archiv für Naturgeschichte* for 1839.

The *first part*, which is dedicated to the natural history of the oyster, contains, besides much that is already known, also much that is new, by which previous statements are enlarged, restricted, and partly corrected. In the Jütland oysters, the author found six pearls, two of which were of the size of peas, and the others of small shot; but, generally speaking, they are rare, and of small dimensions. In the oyster-banks of Schleswig, the *Ostrea hippopus* occurs along with the *Ostrea edulis*; but, as its taste is inferior, it is of inferior value as an article of trade. There is no new information respecting the organs of generation. Regarding the period of propagation, it resulted from the investigations of the author, that it does not appear to take place simultaneously. He found, in July and August, individuals which, on opening the shell, contained a milky fluid, which exhibited, under the microscope, very minute but perfectly formed young ones, provided with a thin shell; but such oysters were rare, for hardly one was met with in ten. The opinion, that oysters, at the period of their propagation, are lean and of a bad watery taste, is fully proved to be erroneous; when newly taken out of the sea, their taste is just as good in summer as in winter; and there is no foundation for the belief that the eating oysters in summer is unhealthy. The statement, that oysters only live in such places as are never uncovered during the greatest ebb of the tide, is limited by the author. In northern districts, they cannot endure the cold at low water, and hence they live at a greater depth. But several of the oyster-banks of Schleswig have so low an amount of water, that they are bare during a great ebb, or during certain winds. The author has ascertained similar facts on the Norwegian coast. On the west coast of Schleswig, it has often been remarked, that, in summer, oysters occupy spots which are even frequently laid bare, and that the oysters in these places can thrive a considerable time when the winters are mild; but if a frost take place, they speedily succumb. An examination of the position of the oyster-banks of Jütland and Schleswig does not confirm the idea that oysters flourish more especially at the mouths of rivers. M. Kröyer very correctly remarks, that we are not to regard oyster-banks as elevated portions of the submarine land, or as rocks or sand-banks, &c., to which the oysters are attached by their valves; but merely to understand by them those parts of the bottom of the sea on which oysters occur in large numbers. Where the submarine land consists of rocks and loose stones, the oysters adhere partly to the projecting portions of the rocks and to the separate stones; but many likewise lie loose on the bottom: the latter is, of course, always the case when the bottom consists of loam, sand, or mud, except when several have grown together in irregular heaps of three, four, or five individuals. More than five or six are not united; because, were they to grow together in too large masses, the lowest would be prevented, not only from being developed, but likewise from opening their shells. It is likewise not correct that they always rest on the valve which is turned downwards. The circumstance of a much larger number of young individuals not being met with on the banks of Denmark, the author is inclined to attribute to the numerous enemies of the oysters, of which the worst are the voracious sea-stars. *Cliona celata* of Grant is so far prejudicial to oysters, that its groups penetrate the shells, and pierce holes in them, thus rendering them soft and brittle, so that their inmates are deprived of their protection, and more exposed to their enemies. Such pierced oysters are also not willingly taken by dealers, as they are easily broken in the packing. The author discusses the circumstances that are favourable and unfavourable to the prosperity of oysters, and states, as the place best adapted for their development, a flat, firm bottom, at a depth of from five to fifteen fathoms, where the current is not violent. Too strong a current carries away the young brood: a flat bottom, and an inconsiderable depth, facilitate the fishing.

The *second part* treats of the Danish oyster-banks in particular. The term Danish is employed in the Danish sense, inasmuch as the banks of Schleswig are included under it, the oysters from which are known throughout Northern Germany by the name of Holstein oysters. The inhabitants of the Schleswig-Holstein duchies will not be altogether pleased; and their provincial assemblies may perhaps regard this treatise as an insult to their nationality, just like the map of Captain Olsen, which represented the duchy of Schleswig incorporated with Denmark, under the name of South Jütland. But joking apart, the author may answer for this to the natives of Schleswig. It is a matter of indifference to us whether these banks are termed Danish or Schleswigian, so long as they continue to afford us good Holstein oysters, and while we know that of all the banks belonging to the Danish crown, they are the most productive and the most numerous. Their number amounted to fifty-three; but several of them have been abandoned, being partly sanded, and partly exhausted, so that now there are only about forty which are available. They lie on the west coast of the duchy of Schleswig, nearly opposite the line of coast between Tondern and Husum, between the small islands Sylt, Amrom, Föhr, Pelworm, Nordstrand, &c. Almost all these islands are surrounded by shallows, termed *Watten*, which are dry at low water. These watten are traversed by deep channels, in which, or on whose slanting edges the oysters occur. The largest and richest among the available banks is Huntje or Huncke, to the east of the island Sylt; and its oysters are excellent in quality. Unfortunately, however, this bank has but a shallow covering of water, and suffers in severe winters. In the winter of 1829–30, on this bank alone, there were more than 10,000 tons of oysters, or about eight millions of individuals destroyed by frost; although this estimate is perhaps an exaggerated one, as it was furnished by a person in the service of the overseer belonging to the lessee of the fishery.

The Danish banks, properly so called, lie on the east side of the northern extremity of Jütland, opposite Skagen. It may be said that they stretch along parallel to the east coast of the peninsula of Skagen, from its northern extremity to Hirtsholmen, and in such a manner that the fishing village Aalbeck lies opposite their middle. Three banks are enumerated, viz. the lower chiefly to the north, and opposite Skagen; the middle, opposite Aalbeck; and the upper, to the south. According to some statements, these banks reach down past Hirtsholmen, east and west, round Läsö to Anholt. According to statements made, oyster-banks would seem also to extend round the west coast of Jütland to Hirtshals. It is only the banks lying on the east coast opposite Skagen which are let and fished. Their produce is much smaller than that of the banks of Schleswig; and their sale is limited to Jütland itself and Copenhagen; whereas the oysters of Schleswig are exported to Hamburg, and thence over the whole of Northern Germany; and formerly, likewise, they were sent to all the sea-ports of the Baltic, as far as Reval and Petersburg. Of late years, however, the English and Dutch oysters have done them great injury, even in Hamburg, which is now the great mart of the Schleswig oysters. The term *deputat*-oysters (*i. e.* oysters forming an allowance made to certain persons at the public expense), generally applied to the best Schleswig oysters, had its origin in the circumstance, that the lessee of the oysters was not only bound to deliver twenty-five tons to the royal kitchen, but likewise from 1000 to 3000 individuals to the privy councillors, the presidents of the public boards, and a number of other official persons, which amounted altogether to 56,000 individuals, or seventy tons. The lessee was likewise responsible for the goodness of these oysters; for, in the historical portion of the work, we find that, on one occasion, a lease was not lengthened to a lessee because he had sent bad *deputat*-oysters to Copenhagen. There is no wonder, therefore, that this appellation was bestowed on the best kind of oysters. The lessee is bound to give up the banks in as good a condition as they were when he received them. To effect this object, the banks are examined by an official commission; and they are fished at three fixed points by fishermen who are bound by an oath. The condition of the bank is determined by the quantity of oysters taken. The results of the various examinations from 1709 to 1830 are given by M. Kröyer in two tables. They lead to the conclusion that the productiveness of the banks has diminished in an extraordinary degree; and that if it should in future diminish in the same proportion, there will soon be no oyster-banks in existence in Denmark.

On the Occurrence of a Fossil Bird in the Chalk-Slate of the Canton Glarus. By M. HERMANN V. MEYER.

The *Ornithichnites* made known by Hitchcock in North America, can afford no proof of the occurrence of birds in rocks antecedent to the tertiary period to me, who am a decided opponent of the view, that the appearances presented by the older sandstone formations, which have made so much noise at the present day, and which undoubtedly deserve attention, have been certainly produced by the footsteps of animals. There is also, however, the fragment of bone from the Hastings-sand of Tilgate forest, which has been examined by Owen, who has declared it to be the tarso-metatarsal bone of a wader, resembling a heron, on account of the bone presenting an oval spot, denoting the articular surface or place of attachment of the posterior or opposable toe, and of the indications of the longitudinal ridges of bone, which, in the metatarsals of birds, afford attachment to the aponeurotic thecæ, that tie down the tendons as they glide along the metatarsus of the toes.* But in this bone the actual lower extremity of the bone is awanting, and the other bones found along with it seem to have belonged to a Pterodactylus, rather than to birds, although they have been referred to the latter. Besides these bones, there is preserved in the collection of the Academy of Natural Sciences of Philadelphia, a fragment of bone found by Mr S. W. Conrad, near Arneytown, New Jersey, in a friable green marl, considered as belonging to the greensand formation by Mr Morton, and which is regarded by the latter as the tibia, but by Mr Harlan as the femur of a bird of the genus Scolopax.† But respecting this last bone, there are neither figures nor a particular description, by means of which the necessary comparison can be made.

Thus the important question as to the occurrence of the remains of birds in formations antecedent to the tertiary series, was by no means satisfactorily determined, and in consequence of the striking approximations to birds, lately discovered by me to exist in the Pterodactyles,[*] I had so much the more reason for believing fossil birds to be confined to the tertiary strata.

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* Vide description and figure in the Geological Transactions, 2d series, vol. v. part 1st, p. 176.

† Vide Morton's Synopsis of the Organic Remains of the Cretaceous group of the United States, p. 32; and Harlan's Medical and Physical Researches, p. 280.

I was therefore not a little surprised at the sight of a slab of Glarus slate, lately sent me for examination by the kindness of M. Arnold Escher von der Linth of Zürich. This rock has been rendered remarkable by its fossil fishes, and by its Chelonia Knorrii; it was formerly, on account of its petrographical characters, considered of great age, but subsequently, from the nature of the fossil fishes it contains, has been determined by Agassiz to be a formation of the age of the chalk. On the slab forwarded to me, I found the remains of the skeleton of an animal, which can have been nothing else but a bird; and this view is placed beyond all doubt by the distinctly preserved bones of the wing and the foot. The feet cannot have been adapted for wading, and the bird cannot, therefore, have belonged to the *Grallæ*; it would seem rather to have been one of the *Passerinæ*, and it must have had the size of a lark.—*(From Leonhard and Bronn's Jahrbuch*, 1839.*)*

On the Influence of Weather in Relation to Disease. 1. *Temperature;* 2. *Moisture;* 3. *Pressure;* 4. *Electricity.*[†]

1. *Temperature of the Atmosphere.*—The influence of temperature is the most familiar of the conditions we have to examine, and that which has been chiefly studied; in its direct effects on the body, and indirectly through the diseases of climate. As respects the former, it seems certain that changes, sudden or frequent, are principally concerned in these results.[‡] The

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[*] In regard to the air-cells in the bones, see the *Jahrbuch für Mineralogie*, 1837, p. 316; and respecting the structure of the anterior extremities, the same work, 1838, p. 668.

[†] This interesting article we have taken from a volume of rare merit lately published by an accomplished physician, entitled "Medical Notes and Reflections by H. Holland, M.D. F.R.S., Physician Extraordinary to the Queen," &c. &c. 8vo, p. 628. Longman, Orme, & Co., London.—EDITOR.

[‡] *Αἱ μεταβολαι μαλιστα τικτουσι νουσηματα, και αἱ μεγισται μαλιστα.—Hippocrates.*

power of accommodation in the body, depending on the generation of animal heat, and on the functions of the lungs and of the skin, provides in the healthy state against all which are not in excess. But where these functions are impaired, or the body otherwise disordered, every such change has influence; either by disturbing the balance of circulation between the external surface, and the membranes or different glandular structures within the body;—or by checking or augmenting the discharge of perspirable matter;—or in part, it may be, by more immediate action on the nervous system; though of this we have less certain proof.

The degree in which external cold may alter the balance of circulation,—directly, by contracting the capillaries and smaller arteries of the surface; or indirectly, by the effect of this altered balance upon the action of the heart itself,—is scarcely enough regarded in its various details.* It is to be presumed, on the most common grounds of estimation, that the differences thus made may vary, (according to the degree of cold and the powers of re-action from within) from the smallest assignable amount to that of several pounds of blood, changed in its manner of distribution through the vessels of the body. The importance of such fluctuations must be obvious on the most general view. And they include, it may be added, not merely the repulsion of blood from the surface by the contraction of the capillaries, but also the effects of the re-action and return of blood to the part; the latter consequences often very remarkable in their influence on the bodily functions.

The tendency of sudden changes of temperature to produce topical inflammation, is doubtless owing chiefly to these disturbances in the balance of circulation, which arise from changes, general or partial, in the capillaries of the surface. Rheumatic affections, whether inflammatory or not, are usually attributed to the same cause; rightly, as respects some states which bear the name; not so, as to others, which are undoubtedly of different origin. Many disorders of the serous and mucous membranes, of the lungs, of the alimentary canal, and other viscera, depend more certainly on changes in the distribution of blood thus made; either suddenly, or by continuance and repetition. And these also are among the changes which have direct influence on the brain; the result of various averages shewing that apoplectic seizures are most frequent when either heat or cold are severe in degree;—the mode of action doubtless different in the two cases; yet in each depending principally on disturbance excited in the movements of the blood.*

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* The experiments of M. Poiseuille, in his treatise on capillary circulation, confirmed by those of M. Magendie, show the effect of a low temperature in retarding or preventing the passage of blood through these extreme vessels.

The influence of external temperature on the functions of the skin, whether those of transudation or simple evaporation, is scarcely yet fully estimated; though the researches of Dr Edwards and others have done much to extend our knowledge on the subject. The changes so made, either in augmentation or diminution of the natural discharge, are obvious and often very great. Without reciting the observations directed especially to these points, it may be remarked that a natural provision against injury exists here, as in the case of the temperature of the body, in the diminution of other excretions, and in the relation of absorption to the matters perspired;—a remedy inadequate, indeed, to repair extreme or continued losses, but sufficient for all the ordinary occasions of life. This subject belongs, subordinately with that of heat, to the general doctrine of climates; the influence of which on the animal economy is regulated, in part, by the provisions just named; in part, by actual changes in the state and texture of the integuments of the body; exclusively of those modifications which depend on the usages of life in each country or community.

The effects of perspiration suddenly checked by external cold, are the subject of general apprehension, and influence many of the details of medical practice. Though in some instances mischief may arise from this source, I believe the alarm to be unwarranted in degree; and many of the effects, so attributed, to be due to other causes acting concurrently; such as exhaustion from fatigue, the perspired fluid left on the body, and the influence of cold itself in suddenly changing the balance of circulation between external and internal parts. The latter effect may equally happen, independently of perspiration; and there is no ascertained reason why this, previously occurring, should alter or change its amount. The customs of some countries, and the necessary habits of particular avocations, shew how suddenly these changes may be made without any injury, if other causes of mischief are excluded; and prove the uselessness or wrong selection of many of the cautions current on the subject. This is a point on which just views are very desirable to the practitioner. It is in every case important that his judgment should be unfettered by common opinions, exaggerated or unproved;—and though here, as in other instances, it may be well to concede sometimes, yet must he ever maintain the prerogative of applying his better knowledge, when circumstances require it.

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* Regarding the *Coup de soleil*, usually cited as one of the most striking examples of the effects of heat on the brain, we have some recent evidence (though hardly decisive), to show that the change, thus suddenly induced, belongs rather to the pulmonary circulation, than directly to the head. This is contained in a paper by Mr Russell, surgeon of the 68th Regiment, at Madras; read before the College of Physicians two years ago.

To the more common results of variations of temperature, hitherto noticed, may be added those which depend on extremes of heat or cold, suddenly, or continuously, applied to the body; the observations regarding which have been much extended of late years. The recent voyages of northen discovery furnish many as to the effect of high degrees of cold, of great interest to physiology; but as these are now familiar, and do not apply to practice, I merely allude to them as one portion of the inquiry.

There are other parts of this subject, more practical in kind, which, though better considered now than formerly, do yet not receive all the notice they deserve. Such are the direct applications of cold as a remedy; possessing certainly great value, and admitting of much more general and defined use than is made of them. Common prejudices, fostered to a singular degree on this point, are not only a great hinderance to the physician, but often do much to pervert his own views and practice. Accordingly we find that the effect produced for a time by the writings of Dr Currie, on the application of cold in fevers (exanthematous as well as others), has been only partially sustained; and that the common course of treatment scarcely goes beyond the removal of the old and noxious errors of close atmosphere, hot rooms, and thick clothing;—doubtless a very beneficial change, but not precluding the more direct and extensive application of cold to the surfaces of the body.* Whatever the theory of this action, the benefits gained are incontestible;—familiar to all who have fairly employed it, and well recognised by patients themselves. Almost may it be taken as a rule, that wherever there is a hot and dry skin, cold in one degree or other may safely and expediently be applied to change its state. The benefit of simple abstraction of heat is great in such cases; and the fact is not sufficiently adverted to, which I have often put to thermometrical test, of the extent to which this influence is diffused beyond the surface to which the cold is immediately applied. There is no real risk here to countervail the good gained. We are sedulous in providing for and varying the application of heat to the body; while, from one cause of alarm or another, little provision is made for the opposite remedy, though not less capable of being actively and beneficially employed.

A point subordinate to this, which has had less notice than its practical importance deserves, is the influence of cold or hot air respectively, upon wounds or open surfaces. The greater sensibility of parts so exposed, and the more direct actions on their vascular texture, make this condition a very important one. And accordingly we have much proof in private practice—still more from the experience of hospitals and military campaigns—of the effects produced by heat and cold severally, or by changes from one to the another. I have seen this remarkably in the army hospitals in Portugal; where in summer, the general rate of recovery from wounds was accelerated or retarded, as the temperature became suddenly cooler, or the reverse. It is singularly attested in the instance of the wounded, left exposed on the field of battle. Though the better understanding of ventilation has contributed towards this object, yet might much more benefit be derived from the direct effects of cold as an antiphlogistic means; either through the atmosphere, or by immediate application to parts affected. The employment of cold water externally, as a dressing to fractured limbs, gives one proof among many of the benefits of the latter practice; and we have reason to infer, that the liquid form is the best in which such application may be made for the relief of inflammation in open wounds, or other inflamed surfaces. Here also prejudices are to be overcome; the best assistance towards which is often derived from the sensations of the patient himself.

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* I say *surfaces,* because in fact cold acts remedially on the lining of the alimentary canal, as well as on the outer skin; is often as imperatively required by the sensations of the patient; and not less sanctioned by the good obtained.

While thus briefly referring to some of the effects of temperature, and chiefly on points of practical import, it must be repeated, that we can rarely view them separately from the other conditions before noticed. Every change as to heat or cold in the atmosphere must either be the effect of, or produce, other changes of atmospheric state; and none of these, it may be affirmed, are wholly indifferent to the body. Even in the simple case just mentioned, of the influence of warm or cold weather on open sores, though the atmosphere be admitted as the source of change, the effects are probably not due to temperature alone. Still less can it be supposed in regard to certain winds of our own climate; such as those from the east and south; the relations of which to the body are in no wise interpreted by the thermometer. The same remark extends more remarkably to the Sirocco of the South of Europe; and generally, perhaps, to the dominant or more peculiar winds of every locality over the globe. Where any one is especially noxious in producing epidemics, or in its effects on the general health, there, probably, is the direct influence of temperature on the body least in proportion to the other causes concerned.

Even in the endemic diseases of particular climates, the same view may be entertained. We have no certain proof that the fevers of the West Indies, or the Guinea coast—or the dysentery, remittent fevers, and liver diseases of different parts of India—or the malaria-fevers of Italy and Greece, are owing to the heat merely of these several climates. Hepatic disorders, indeed, generally, may be considered as having closest connexion with this influence; but in others of the above examples, the best evidence we possess leads us to causes, in which temperature is only indirectly concerned.* And though this evidence be notoriously imperfect, yet is it valuable in the direction thus given to further inquiry. We have no direct cognizance of those miasmata, whether of animal or vegetable origin, or simply chemical in kind, which form the material of epidemic disease; but we know that such material emanations exist; that they differ in different localities; and that variation of temperature is the condition seemingly most essential to their several forms and various activity. We have evidence, both experimental and of natural occurrence, of the effects of a certain degree of heat in producing or evolving these agents; and of a higher degree in destroying them, or suspending their action. Such results might be inferred as probable, from what we have cause to presume of their nature; looking here, as the nearest analogy, to the chemical constitution of the known poisons, whether of animal or vegetable origin,—to the feeble affinity by which their elements are generally united,—and the facility with which they are decomposed, and enter into new combinations from slight changes of temperature alone.

I need not refer to the many illustrations of this subject furnished by the history of disease. They are continually multiplied, as observation becomes more exact; and it is likely that the estimate of effect from this source will enlarge in proportion to our knowledge. The unequal influence of equal averages of heat in different localities might itself suggest doubts whether too much is not attributed to its direct action, too little to its operation through other agents. All examination of particular local conditions, such as soil, elevation, general humidity, quantity and kind of vegetable growth, manner of culture, and extent of running or stagnant water, shews the singular importance of these circumstances, as determining the endemic disorders and average health of different localities, exclusively even of the habits and employment of the people in each. Every country and district furnishes such instances; and all concur in proving that we must estimate the influence of temperature upon the body, and especially of heat, subordinately in great part to these more varied conditions. Isothermal zones would afford a very uncertain measure of the character or prevalence of disease.*

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* Here I may again refer to Dr J. Johnston's book on the Diseases of Tropical Climates, in which he shews how vaguely these relations of disease and locality are often considered, and made the subject of inference. We speak of hepatitis and remittent fevers as diseases of India, without adverting to the fact that the true hepatitis (or that which is not a sequel to fever) is ten times more prevalent on the coast of Coromandel, than in the plains of Bengal;—intermittent and remittent fevers in an equal ratio more frequent in the latter locality. The medium annual temperature of Madras is known to be amongst the highest on the globe (88° Fahr.); that of Calcutta about ten degrees lower.

But it is a further question here, whether variations of atmospheric temperature may not induce a state of body, rendering it more liable to receive specified infections, however generated by agents without? That there are such differences of bodily condition, however vaguely known to us by external signs, must be admitted. And it is perhaps not a rash inference from the temporary effect of exposure to great heat, in quickening the circulation and augmenting the animal temperature, that continued exposure to the same cause, even much less in degree, may keep the constitution in a state prone to morbid actions, when the exciting causes are present. The uncertainty in this case depends in part on our ignorance of the equality of the causes, and of the relative degree of exposure to them; and can only be met by strong presumption, or actual observation of change in the bodily state. But I think it improbable, seeing especially the small increase of animal temperature from elevation of that without, that heat alone is concerned in producing such alterations: and, if depending on atmospheric causes, it is likely that these are of mixed kind, and blended with other actions more peculiar to the body itself.†

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* Many excellent papers on this subject have appeared of late in the Transactions of our provincial Medical Associations, based on that statistical method which alone is capable of affording sound results. They all shew the intimate relation between the nature of the surface and the prevalence or infrequency of particular diseases in given localities; a point in which external temperature is only indirectly concerned, but where the effects are of singular importance in a practical view. Long and careful averages can alone be effectual in expounding them, by removing gradually all extrinsic or accidental circumstances.

† The best observations we possess, shew that the change of temperature in the human body, made by extremes of natural climate, does not exceed

The action of cold, regarded in the same general light of locality or season, is perhaps less remarkable than that of heat, as not equally involving those physical agents which become the direct causes of disease. But, besides its effects on the balance of circulation already noticed (and which, though more strikingly shewn by sudden changes of temperature, are also a result of continued cold), we have to notice its indirect influence in producing certain habits and necessities of life which variously affect the health; and more especially the alteration it makes in all that relates to food in those countries, where it gives the predominant character to the climate.

The same manner of reasoning on the morbid effects of heat and cold, whether immediate, or such as depend on long exposure, must lead us to make large allowance for the momentary condition of the body, and the general habits of life. A man under strong exercise, or with habits of such, is very differently affected from one in repose. Protection from, or exposure to, the causes which augment the direct influence of temperature, as the open sun, wind, and rains,—comfort or privation in the manner of life,—habits of temperance or sensual excess,—even the different occupations and temper of mind;—all these conditions modify more or less the effects of heat and cold on the body; and some of them, in particular cases, so powerfully, as almost to invert the accustomed results of such exposure.

In practice also, and for a rule in the habits of life, regard is not sufficiently paid to the different power which different individuals possess, of generating animal heat. This function, whether depending on changes in the blood and manner of circulation, or more directly on the nervous system, is as various in its power and exercise as any other of the body, and requires

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one or two degrees. The experiments of Berger and Delaroche, on the effects of exposure to higher and more sudden heat, prove that a temperature of 80° Fahr. above that of the body may raise the animal heat eight or ten degrees; a grade still below that evolved in some fevers, and under particular lesions of the nervous system. It is important to notice, that the same conditions produced different results in the two experimentalists; an effect that might have been anticipated, seeing its probable dependence in part on the excitement to circulation, which is so various in different individuals from the same causes.

to be dealt with as such. Each age, too, has its changes in this respect, as well as every condition of health; and precautions founded on them cannot expediently be neglected, provided they are not so minute as to interfere with other parts of the economy of life, equally essential to the welfare of the whole.

In a brief outline like this, it is needless to particularise instances. They are familiar to common remark; cited in medical works (though not always so specifically as the subject requires); and are very striking in the more extreme cases, where the struggle between the agency from without, and the powers of resistance from within, is most strongly marked. For we must ever revert to those great provisions in the constitution against all extreme or sudden changes of external temperature, by the laws which govern the production of animal heat; the action of the exhalants of the lungs and skin; and possibly also the secretions of other organs.* No correct results can be obtained as to the agency of heat and cold upon the body, without keeping these powers of balance constantly in view; and as they again are perpetually undergoing modifications from the various conditions of life, so is there a circle of relations, tending altogether to equality of average, though greatly broken and interrupted in its several parts.

2. *Moisture of the Atmosphere.*—We have no evidences of equal provision, as respects the second of the general conditions of the atmosphere; viz., its hygrometrical state.† But, on the other hand, there is every reason to infer that no similar need exists for it. The simple agency upon the body of dry or humid air, is doubtless much more limited in every sense than that of heat and cold;—restricted, as far as we can see, to certain organs, and less powerful in its influence on these. It is still more difficult also to detach it in observation from the influence of other causes. Sudden and considerable changes

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* Taking the record, seemingly well authenticated, of the two extremes of temperature of the human body, as determined by diseases affecting the blood, we find them to include a range of nearly 40° of Fahrenheit.

† Unless, indeed, we admit as partially and indirectly such, the apparent relation between perspiration and absorption; the latter process balancing, by its increase or diminution, any changes the former may undergo from the different conditions of the atmosphere as to moisture or dryness.

do not occur in the hygrometrical state of the air, without corresponding changes in its temperature, weight, and electrical condition. Even the common fog, or mist, is far from being a single or simple phenomenon. In some instances it is the cloud already formed, and brought by currents of air or other causes to a lower level;—in other instances, as in the fogs which occasionally intervene between thunder-storms, the result apparently of a change going on in the electrical relations of the earth and atmosphere at the spot, producing alterations in the hygrometrical state of the latter. Science has not yet assigned their proper place to these several changes, as regards the relation of cause and effect. But however this be determined hereafter, the complex nature of their action on the body still remains, and will long retard any certain conclusions on the subject.

Another source of ambiguity, in considering the effects of different degrees of humidity of the air, is the influence of local circumstances of soil and surface in modifying this; especially in that lower stratum of the atmosphere, with which man has chief concern:—and this modification regards not merely the quantity of water taken up by the air, or precipitated from it, according to the several conditions of the surface, and the action of external sources of temperature; but also the various miasmata disengaged, or otherwise acted upon, by the same processes. I have already adverted to these material causes of disease, in their more particular relation to heat. Whatever their nature (and we have every thing still to learn here), it seems certain that the presence of moisture, either on the surface or near it, under the form of vegetation, damp air or soil, and acted upon by a certain degree of temperature, contributes much to their production, if not indeed essential to it. And to these conditions, conjoined perhaps with the electrical state of the atmosphere, we may chiefly attribute the greater unhealthiness of the rainy seasons in tropical climates, which the mere quantity of rain falling will not equally explain.

But further than this, there is some cause to presume that aqueous vapour in the atmosphere, whatever its mode of combination, is much concerned in giving activity and spread to these miasmata as the cause of disease. It is idle to speculate upon physical relation here (whether that of solution, or of independent elasticity, according to Dalton's theory of vapours), while so entirely ignorant of the chemical constitution of these agents. We can only affirm that the conditions which concur to their production, are likely to aid their diffusion and action on other bodies: and though the proofs are by no means assured, yet there is evidence that a foggy and humid state of atmosphere is that in which contagious or epidemic diseases are most readily and extensively spread. Other causes, however, doubtless operate, and produce many apparent or real exceptions to the fact.

Recurring to the more direct influence of air, loaded with moisture, on the body, we have reason to expect it to be greatest on the functions of respiration and of the skin; and observation, as far as it goes, confirms this view. The effects in each case are probably owing chiefly to the altered amount of discharge from the exhalant vessels of the organs concerned; in part also, especially when the external temperature is low, to the greater effect of cold, conjoined with moisture, on the capillary vessels and sentient extremities of nerves of the surface exposed. The difference to the feelings between a temperature of 45° Fahr., in dry or in damp air, is one which cannot escape the most ordinary attention. It is a difference equally marked as that between steam, and air heated to 212°, in their respective application to the body. The membrane lining the air passages is obviously most liable to these effects and to disorders depending upon them; as the experience of patients suffering under asthma and bronchitis, however varying in details, painfully testifies in its general results.

For the reasons already given, we are rarely indeed entitled to speak of humidity alone as a morbid cause; but it undoubtedly concurs with and renders others more effectual. And in the case of very damp air received into the lungs, it is probable that it may act expressly by retarding or impairing the changes made in respiration; and especially those depending on exhalation, which form so important a part of this process. Modern research, in shewing the facility with which these changes take place (not merely by vascular structure, but through intervening membranes, and in dependence on more general physical laws), exposes in the same ratio their liability to be altered or impeded by causes, which before scarcely came into our view.

The action of very dry air on the body is even less certainly known to us. There is reason to believe that the effects of the Simoom wind (exaggerated, perhaps, in common narrative) are due in part to this cause;—in conjunction with its singular heat, the quantity of minute sand it conveys, and above all, the electrical condition of the current air. There are more familiar reasons, however, for presuming that the atmosphere may occasionally be too dry (becoming so either naturally or by artificial means), for the healthy state of the functions of the skin and respiration. Without referring to the question, still undecided, whether absorption of atmospheric moisture through the surfaces of the body does occasionally or habitually take place as a natural process,—and without affirming that the effect is derived from pulmonary evaporation unduly increased,—we have various proofs that a state of air is often created by artificial heat, insalutary to the body; and that this condition may be removed by means which restore to it a certain degree of humidity. Houses or apartments heated by stoves (particularly under the style of domestic architecture in England), are liable to suspicion on this score; and if the fact be ascertained, which is not difficult with the better hygrometers now in use, it becomes expedient in every case to remedy it; either by exposure of a surface of water for the benefit of slow evaporation, or by other means. What is merely an inconvenience for the hour or day, may pass into a serious injury to the health, when there is long-continued exposure to it.*

3. *Pressure of the Atmosphere.*—The influence of the atmosphere in producing morbid conditions of body, through its

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* In a paper read before the Royal Society in 1836, on the ventilation of the Custom-House of London, Dr Ure states the peculiarities of atmosphere in the Long Room, warmed with hot air, and where 200 persons are always present, to be its extreme dryness (sometimes 70 per cent. of Daniell's hygrometer), and negatively electrical state;—the general effects produced being vertigo, with a sense of fulness and tension about the head; a quick but feeble pulse; and deficient circulation in the lower extremities.

changes of weight, is a curious subject of inquiry in many points of view. It is chiefly and most familiarly noted, in disturbances of the balance of circulation throughout the body; and particularly in that of the head and lungs; from obvious causes as respects the economy of these organs. The functions of the lungs, indeed, are subject to this influence in several ways; even the mechanical part of the process being in sone part concerned; as well as the balance between the external air and that within the bronchial cells; and the relation of the whole to the quantity of blood in the pulmonary circulation. While the action of the heart must necessarily be affected by all which thus tends to disturb the equal movements of the circulating fluid.

Another consideration again regards the relative effect of air of different density, in producing the proper changes on the blood. It is clear that there exists a point of rarefaction, at which this quantity of oxygen is insufficient for the purposes it has to fulfil. Or, giving the statement its most general form, there must be a particular specific gravity of air (concurring probably with the medium barometrical pressure), which is best fitted for the necessities of the function; and all deviations from which, in one degree or other, interfere with the completeness of its performance.

These effects, however, under ordinary circumstances, and in healthy state of body, are slight or inappreciable in amount; limited by the range of barometrical variation, and by the usual slowness of the changes taking place. They are augmented of course when the variation is more rapid and of greater extent;—still, however, depending on changes in the state of respiration; and on irregular distribution of blood, from the altered balance of pressure between the external and internal parts of the body. The latter cause might be expected to affect most the vascular system; seeing its structure, functions, and the mechanical principles which in part determine the motions of fluids, even in the vessels of the living body. The common observations with the air-pump and cupping-glasses shew the facility with which these vascular textures, and the contained fluids, yield to any such change of balance; and in the effects in the diving bell, on the head more especially, produced by an increase of only one-fifth, or one-sixth, in the atmospheric pressure, may be received as proof, though less obvious, of the same fact.*

But in less peculiar cases than these, notable effects may occur, when the changes in the weight of the air are frequent, sudden, and considerable, even within the ordinary range of atmospheric variation. Regarding merely the average pressure upon the whole body, it is to be supposed that any very sudden fluctuation, to the amount perhaps of one-thirtieth, may produce temporary changes in the balance of circulation between external and internal parts, of much moment to the latter. And these are particularly to be looked for, when there is individual liability to certain diseases, or close approach to them at the time ; a point requiring to be kept in mind more than it usually is, in estimating the influence of exciting causes, whatever their nature.

This observation, as I have already stated, appears especially to apply to affections of the brain. I have made note of two or three periods, since I began practice, during which there has been a more than wonted frequency of apoplectic or paralytic seizures within my immediate knowledge ; so marked as to make it difficult to attribute the fact to mere casualty, notwithstanding the many circumstances which tend to invalidate such results when not verified by large averages. The same fact, observed by others, has generally been attributed to external heat alone. But allowing what has already been assigned to this cause, the particular character of the weather at these times will scarcely support the inference ; nor has the result in question been equally apparent even under higher degrees of atmospheric temperature. While, on the other hand, I have observed at these periods frequent and rapid changes in the barometer, often with great depression of its level ; and have noticed at the same time the very common occurrence of lesser affections of the head,—vague and uneasy sensations, oppression, vertigo, and what may be termed a feeling of want of proper balance to the frame—all indicating some cause present which tends more or less to disturb the equality of circulation through this organ.

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* The suggestions of Sir James Murray (*Report of the British Association,* 1835), for the use of artificially rarefied or condensed air, in application to the surface of the body as a remedial agent, deserve much attention. The cases are numerous, where changes in the local distribution of the blood, thus readily made, would be of much value in the treatment of disease.

In fact, the ordinary phrases of heaviness and lightness of air (however misplaced or even inverted their use) prove the general consciousness of these changes in their slighter influence on the body. It may be difficult to say through what organ or function this feeling is chiefly conveyed; but probably it is a compound effect of the changes in circulation, in which the sensorium, the lungs, and the muscular system, all participate. Even the organs of digestion seem to be affected, directly or indirectly, by the same causes. Without referring to the doubtful instance of vomiting produced in highly rarefied air, I think I have observed frequent disturbance both in the sensations and functions of the alimentary canal, under any rapidly diminished weight of the atmosphere, or where its changes were more frequent than usual. I have remarked in the preceding chapter on the indications of disturbance to sleep from the same cause.

All these inferences, however, are rendered uncertain by the great difficulty of simplifying the conditions which belong to them, where the physical causes concerned are so unceasingly blended in their operation. It may be, for instance, that what is attributed to changes of weight of air, really belongs to electrical changes in the atmosphere, producing or attending the former. Another more familiar case of ambiguity, is that of the sensations experienced in reaching a high mountain-summit. Though often attributed to rarefaction of the air breathed, I doubt not (on my own observation as well as that of others), that they are chiefly owing to the expenditure of bodily power that has been incurred by muscular action, hurried breathing, and quickened action of the heart. These sensations in great part subside, when the immediate causes of lassitude and disorder are removed. Or, if we yet need explanation of that singular sense of fatigue in the limbs, which is alleged to occur when walking in elevated regions, even without the toil of ascent, we may perhaps find it in a suggestion of Humboldt; whose sagacity is ever awake to all natural phenomena, even such as pass unheeded by others from their seeming familiarity.

He conjectures that this sensation may depend on the mechanism of the joints and equipoise of the bones being disturbed by the low atmospheric pressure; and the experiments of the two Webers, recently made at his suggestion, have afforded a singular confirmation of this idea.*

The observations in ascent by balloons, now become so familiar to us, shew, even unexpectedly in degree, the extent to which the body can undergo the most sudden changes of atmospheric weight. without any very obvious effect, where the health is unimpaired, and no causes of bodily fatigue are conjoined. In the note below, I have stated some facts derived from the best authority we now possess on this curious subject.†

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* Poggendorff's Annalen für 1837, No. I. These experiments, made upon the hip-joint after the two bones had been detached by cutting the capsular membrane through, shew that the pressure of the air will still retain the head of the thigh-bone firmly in the socket, from which it sinks down when the air is artificially rarefied underneath; the joint thus becoming a sort of air-pump, in which the head of the thigh-bone acts as a piston. [We published an account of the above-mentioned very curious experiments, by Professor Wilhelm Weber, in vol. xxv. of this Journal, p. 254. We would also refer our readers to M. Boussingault's account of the effects of the air of high elevations on the human frame, published in this Journal, vol. xix. p. 98.—Edit.]

† I have been recently favoured with these observations by Mr Green, whose boldness and ability as an aëronaut have given him such general and well-merited reputation. Having now ascended in balloons with more than 400 persons, under every possible variation of height, rapidity, and state of atmosphere at the time, his evidence on the points in question is far more complete than any other we possess.

Mr Green informs me that he has found none of these individuals sensibly affected, otherwise than by the sudden change of temperature and by a noise in the ears, compared by some to very distant thunder; the latter sensation occurring only during rapid ascent or descent of the balloon, and, when greatest in degree, far less distressing than that produced by descent in a diving bell. He has never felt his own respiration hurried or oppressed, except when exerting himself in throwing out ballast, or other management of the balloon, or when suddenly passing into a very cold atmosphere. His pulse is occasionally quickened ten or fifteen beats, but this only when some such exertion has been sustained. He mentions to me expressly, that in no instance have his companions experienced vertigo or sickness; thus rendering doubtful one of the statements current on this subject, and shewing how little the two great functions of circulation and respiration are disturbed, under circumstances where much effect might have been anticipated.

Though the inference is limited to two persons, yet it may be worth while to mention the great experiment made by Mr Green and Mr Rush in September 1838, in ascending to the height of 27,136 feet, or 5¼ miles above the level of the sea; the greatest elevation ever reached by man, and very exactly *corresponding* with the highest ascertained summit of the Himalaya mountains.

These observations lessen any surprise at the great power of accommodation by habit to a constant high degree of rarefied atmosphere. The city of Mexico stands 7,460 feet above the level of the sea; and there are inhabited points in the Andes of Peru even 6,000 feet still higher.* It must be admitted, however, that we have no sufficient knowledge of the diseases in these localities, or of the average rate of mortality, to justify inferences as to effects on the body derived from this single physical cause, when forming what may be termed a constant condition of climate.

On a general view of the circumstances stated, there is reason to conclude that the influence of the different degrees of atmospheric pressure in disturbing the bodily functions and general health, is rather derived from the frequency of fluctuations, than from any state long continued, either above or below the average standard;—that, of the two conditions, suddenly incurred in any extreme degree, the human frame is better capable of withstanding a rarefied than a condensed atmosphere;—and that, in every case, the previous health, and proneness to disorder in particular organs, are greatly concerned in determining the results on the body.

4. *Electricity of the Atmosphere.*—Little though its influence has yet been defined, I believe that the electrical state of the atmosphere is that of all its conditions which has most important and diffused effects on the animal economy; more rapid and pervading than any other; and, as one of the vital stimuli, more intimately allied to the functions of the nervous system. It is that, further, which most closely blends itself, either as cause or effect, with all other meteorological changes; producing thereby many of the difficulties already noticed in estimating their relative amount of influence. When modern science has shewn us that every chemical action is attended by, if not identical with, electrical change;—that the processes of vege-

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The first 11,000 feet were passed through in about seven minutes. Yet, under these remarkable circumstances, Mr Rush suffered no inconvenience but from cold; and Mr Green little other than from the toil of discharging ballast and gas at different intervals, which hurried the respiration during the time.

* Mr Pentland, in 1826, ascertained the height of the town of Potosi to be 13,260 feet above the Pacific. Humboldt mentions inhabited places on the Cordilleras at equal elevation.

tation, as well as those of animal life, involve unceasing alteration in its states;—that no two bodies can be present to each other of different temperature, nor even separate parts of the same body be differently heated, without evolution of this agent;—that every act of evaporation or deposition of water on the surface of the globe has similar effect of change, even the spray of a waterfall sensibly altering the balance of electricity around it;—we may well understand how wide is the circle of these mutual changes, and how important in the economy of nature, including in this the existence and functions of organic life itself.

It is difficult to advert to the effects of atmospheric electricity on the body, either as a vital stimulus or cause of disease, without noticing the question, whether this great natural agent is not itself directly engaged in the functions of the nervous system? If this were eventually determined to be so, the relation of the actions without, to those of the same agent within, would become of still more complex kind, and little amenable to our present means of research. But, taking at present the simplest view of the influence of electrical states of air on the human frame, many circumstances occur, well deserving notice, though yet wanting the certainty needful to give them a place in science.

The natural history of the animal kingdom through its whole extent, furnishes numerous examples (exclusively of those in which there exists an especial organization for electrical purposes) of the singular susceptibility of different animal species to electrical changes in the media which surround them; and many particular cases of instinct, hitherto unexplained, may doubtless be traced to this source. In man the effects are generally less marked, yet nevertheless certain. Without adverting to those singular cases in which the balance of electricity with external objects seems altered by the production of an excess of it within the body, it is obvious that changes of atmospheric electricity have much influence both on the sensations and voluntary powers,* producing results variously analogous to those which attend certain morbid states of body more familiar to us. A few may be noticed in illustration from among those most easily recognised.

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* Of the various instances on record of the curious fact alluded to above, the most remarkable and best attested is that related in the American Jour-

An atmosphere, proved by other phenomena to be highly charged with electricity, produces in many persons sensations resembling those of slight incipient fever; vague alterations of chill and warmth on the skin, general languor of the frame, debility and aching of the limbs, oppression or other uneasiness about the head. In other instances, the feelings created in the muscles of the trunk and limbs have more of rheumatic character; the resemblance being such as to justify a suspicion that some of the muscular affections, often so termed, are actually derived from this cause. In some persons the susceptibility is so great, that even the approach of a thunder-cloud produces bodily feelings akin to those just described, together with a sense of fulness and pricking about the eyes, and a slight tingling over the whole body, which I have often noticed in such cases.

The effects of electricity, artificially applied, may be brought into illustration here. The feelings of numbness or aching that remain for some time in the muscles or joints, after the electric current has been passed through them (whether derived from the machine, from voltaic, or electro-magnetic combinations), much resemble those which occur in the early stage of fever, or under other morbid conditions of the body; while the sensations on the skin which some persons feel in the vicinity of a powerful electrical machine in action,—or by being electrified on an insulated stool, with much conducting material around,—are very similar to others of familiar occurrence, observed especially in certain states of the atmosphere, while electrical changes are going on. And in cases of this kind, there is also a certain degree of languor, or even diminished frequency of the pulse; varying in different individuals, but still uniform enough to prove the reality and nature of the effect.

One of the best tests of the actual operation of atmospheric

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nal of Medical Science for January, 1838. A lady, without any adequate cause, passed suddenly into a state in which she threw out electric sparks to any conductor around her, sometimes to the distance of an inch and a half, with the ordinary sensations attending electrical action; this state continuing for several months, and subsiding by gradual diminution of the power. Other singular details of this case are given, on authority which appears to be good, and without any obvious sources of fallacy.

electricity on the body is, as I think, that mixed sensation of heat and cold which most persons must recollect at some time to have felt,—or rather, the consciousness of sensations which cannot clearly be defined to be either.* Concurrently with such state of atmosphere, which the thermometer does not in any way interpret to us, there generally occurs more or less of the lassitude before described; the muscles are readily fatigued; some degree of headach is often felt; and other vague uneasiness of the bodily feelings, varying much in different habits, and doubtless influenced by the condition of health at the time.

Though these effects are in general more distinctly experienced previously to, or during, thunder-storms, yet are they also sometimes attested in other states of weather where no such storms occur. Certain winds, very common in our own climate, will sustain, even for weeks together, this peculiar character of atmosphere; in degree sufficient to be marked by the results just described, and having still more singular and obvious influence on other animals inferior to man, and on vegetable life. These winds, which may be described, generally, as coming from all eastern points of the compass, but more especially from the quarter lying between north-east and south-east, deserve inquiry under all the aids which modern science can afford. Their various effects on the human body, and on all living organization, are in no wise explained by the temperature or weight of the air. The great dryness of some easterly winds may give better reason for certain of the phenomena, but will scarcely explain the peculiar sense of muscular aching, uneasiness, and languor, they produce in many habits; the almost instant perception of their effects by some, even without any exposure to the external air; and as rapid consciousness of change when they cease. Such sensations belong much more to what we know of electrical agency than to any other cause we can assign; but they need observations more exact than have yet been made, and a careful comparison of these with the physical properties of the winds in question, which future research may also better determine.

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* It is certain that the sensation of itching depends on several different causes acting on the extremities of the sentient nerves; and it seems probable, from various familiar instances, that one of these is the state of electricity on the skin, in relation to that of the air or particular articles of clothing without. If the assertion of Donné be correct, that there is an opposite electrical state of the two surfaces of the skin, it might lead to further inferences on the subject.

Whatever the natural causes which render some of our easterly winds thus peculiar, that from the south-east may certainly be considered to have direct connexion with the Sirocco, as it sweeps with greater or less intensity over the southern half of Europe. This very singular atmospheric current, which, on its more distant border, has probable relation to the Simoom and Harmattan, the hot winds of African desert,—and passes over the Mediterranean Sea under the names of the Levant wind and Sirocco,—reaches England on the opposite side,—its peculiar qualities much mitigated, yet still shewing the same origin in its general direction, in its hygrometrical conditions, and in what I believe to be its electrical influence on animal life. No sufficient explanation has yet been given of these peculiarities, nor are they perhaps definite enough as facts to warrant much theory on the subject. I cannot doubt, however, from my own observations, that the electrical state of these great atmospheric streams, whencesoever derived, is that to which their effects are mainly due. I have witnessed in different parts of the Mediterranean such singular and repeated proofs of this as to give assurance of the general fact, though there are yet wanting the exact determinations required to fix its place in the history of physical phenomena.

Our knowledge of atmospheric electricity is, in truth, still in its infancy. What was written on the subject by Mr Luke Howard and others, at a comparatively early period in the history of the science, is still an authority to which little has been added, in proportion to its progress in other parts. The causes of production, distribution, and change;—its relation to that electricity which circulates in magnetic currents, or otherwise appertains to the earth, or may possibly exist in space beyond the atmosphere;—its connexion with atmospheric heat, moisture, or weight; with the formation of clouds; and the phenomena of wind, thunder-storms, and rain;—and, above all, the relation of its positive and negative states—each one of these conditions is still largely open to inquiry. The latter especially, which has most assured and closest relation to all the rest, is the great mystery still hanging over electrical science; the solution of which would not merely determine these particular questions, but probably, in its connexion with the general doctrine of polarity, enlarge our whole view of the attraction and combinations of matter, whether in atoms or masses, throughout the universe.

What has been thus far said regards chiefly the influence of electrical states of the atmosphere on the sensations and muscular powers. Unless justified in considering as such the occasional effects of lightning on the body, I know no express example of disease which we can affirm to be produced by this agency. Some authors, indeed, have attributed to it certain epidemics of singular character, and not easily referrible to any known cause. But in this opinion they have hardly defined, whether it is to be considered as directly producing the disease, or merely a state of body predisposing to receive it; leaving open still the third contingency of its simply evolving from other sources the virus or material cause of disease. I have elsewhere shewn that it is difficult, if not impossible, to connect these erratic disorders with any state of weather or known quality of the atmosphere; and the reasons derived from their history, apply as distinctly to electricity as to any other property of the element which surrounds us. We must, however, admit the possibility, both as to these and other disorders, of the two latter contingencies just stated. Electricity may be concerned in favouring the generation of malaria, whatever its nature; or it may induce a state of body more liable to be affected by this, or by other causes of disease in activity at the time. We have no proofs on which even to approach towards assurance, but presumption from several sources that this great agent cannot be wholly inert as respects either of the conditions in question.

Though unable then to affirm any one disease to be actually produced by electricity, yet, considering the subject in its whole extent, it is impossible not to see the likelihood of its influence on the body in many ways hitherto undistinguished, or not understood. If a stroke of lightning can in an instant destroy muscular irritability throughout the system,—prevent, in great part, or altogether, the coagulation of the blood,—and hasten putrefaction,—it is clearly to be inferred that lesser degrees of the same action must have definite effects, bearing proportion to the intensity of the electrical changes or transferences taking place. The conclusions, best warranted by the facts we possess, would direct us towards the blood and nervous sytem generally, as the parts of animal economy most liable to be thus affected. The influence of atmospheric electricity on the latter is shewn in the various effects, already mentioned, on the sensations and muscular power; and the proof is greatly strengthened, though indirectly, by the numerous experiments which prove the influence upon these two functions, of electric action from different sources, applied directly to the nerves themselves.* The quantity or tension of the agent, as affecting the body through the air, may be less, and its application not so direct on the nervous system. The low average intensity of animal electricity, as ascertained experimentally, must also be taken into account. But with all these allowances, it is impossible that the effect should be wholly absent or different in kind; and circumstances may often greatly augment its degree, disordering in the same ratio that balance which is most conducive to the general well-being of life.

The same reasoning applies equally to its influence on the blood; and though this part of the subject is even more obscure, yet is there presumption that here the effects occur which are of greatest import in the history of disease. All that chemistry has recently done to determine the nature and relation of parts in the blood (concurrently with that great result which Faraday has established of the identity of electrical and chemical action) justifies the belief that every material change of balance between the electricity without, and that within the body, must have effect on the state of the circulating fluid; transient and wholly inappreciable, it may be, in the great majority of cases; in others, possibly, of longer duration and more extensive in degree. The general relation of acid and alkali, as important in the chemistry of life as in that of inorganic matter;—obvious, not only in the blood itself, but in the materials and processes by which it is formed, and in the secretions and excretions derived from it,—this relation is one in which we have peculiar and constant evidence of electrical agency. The coagulable property of the blood, in whatever it consists, is closely affected by the same cause, even when acting through various intervening tissues. Though we have no equal proof as to the globules, yet their definite form, size, and other peculiarities (necessary as it would seem to the existence of each species), make it probable that they are liable to alteration from an agent, which seems more than any other to determine all definite combinations and changes in the material world.

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* The researches of Humboldt, Müller, Prevost and Dumas, Dr Wilson Philip, Becquerel, and other physiologists, might be referred to in this place; —not equally certain in results, nor conducting their authors to the same conclusions, but concurring to shew the remarkable nature of this agency as a stimulus on the nervous and muscular systems, if indeed it be nothing more.

The tenor and extent of the argument here must be rightly understood. We have no proof of the action of atmospheric electricity, in any of its ordinary states, upon the blood. But the effects of lightning, and the influence of the same principle, proved by experiment in other modes of application to this fluid, warrant the belief that such action may exist; and, if existing, that it must be a frequent cause of disorder throughout every part of the animal economy. We cannot trace diseases with certainty to this source, but how rare are the instances in which we can affirm their real causes! The actual void of knowledge justifies our seeking them through all the new agencies which physical science may disclose; and none is more likely to afford successful results than that now before us.

Two classes of facts, neither of them yet sufficiently examined, are obviously very important to the inquiry. The first includes the indications which diseases themselves may give, in their progress, of alteration of electrical state in the body. The second involves the more general question as to the development of electricity in the animal frame; its natural variations from age, sex, temperament, and connexion with particular bodily functions; and its manner of relation to the electricity of the air without. With the exception of some curious observations of Humboldt and Pfaff on the electrical state of rheumatic patients, we have nothing that approaches to certainty on the former subject. On the latter we possess more results, but all requiring revision and extension by further experiment. We still have no averages sufficient to shew the relative frequency of positive or negative states of the body;—still less what determines this difference, or the changes taking place in the same person.* And this involves directly the question as to manner of relation with the electricity of the atmosphere; one made less difficult, perhaps, since the sagacity of Faraday has reduced all the phenomena of induction to functions of the conducting power; but still requiring much care and research for its complete solution; and a regard, not merely to the changes of state within the body, but to those also ever occurring in the positive or negative conditions of the atmosphere without; of which the comparative excess of positive electricity during the day may be taken as a well marked example.

I have dwelt so far in detail on this part of the subject of the chapter, as being that on which our knowledge is most deficient; and from persuasion also of its future importance in solving many obscure questions in pathology. I might further plead its obvious connexion with all the uses, which may eventually be ascertained of electricity as a remedy in disease; a point where it must be owned that much successful research is needed, to remove that imputation of failure which has been the result of the partial and often abortive trials hitherto made.

Throughout the whole of this chapter, I have been considering the influence on the body of those atmospheric conditions which are commonly termed *weather;* exclusively of all chemical changes in the air itself; of the admixture of other gases; or of the presence of ingredients of animal or vegetable origin, forming the miasma of disease. Even with these exclusions, and merely touching on the several parts of the subject, it will be seen how vast is its extent, and how important its relations to the history of disease. My principal object has been to indicate the latter; and to suggest some of the topics on which more complete knowledge is to be desired. Here, as already remarked, the progress of physical science is ever lending fresh aids to that of pathology; and the unexpectedness of some of the results is the best augury of what may be looked for in future, from the enlarging scope of the inquiry, and the new instruments and means with which it is pursued.

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* The experiments of my friend Professor Pfaff of Kiel, in conjunction with Ahrens, are more complete than any others I know on this subject.

Upon the Colouring Matter of Red Snow. By R. J. Shuttleworth, Esq.[*] (With a Coloured Plate.) (wr)

On the 25th of August 1839, being at the *Hospice du Grimsel*, I learned that some patches of snow in the neighbourhood were beginning to acquire a red tint. The weather for some days previous had been very bad; and quantities of snow had actually fallen, which, at the same time, soon began to melt under the influence of milder weather, and of warm rains. The 24th was a day of thaw and mist; the 25th was clear, the temperature being agreeable, and even hot in the sun, the gentle breeze which prevailed being by no means cold. Accordingly, I hastened to visit the spot, accompanied by my friend Dr Schmidt, and by MM. Muehlenbeck, Schimper, Bruch, and Blind, distinguished Alsace naturalists, who that day, to my agreeable surprise, arrived at the Grimsel.

It was in those places where the snow never entirely melts that we found the patches in which the red snow was beginning to appear. The patches were somewhat inclined, with an exposure towards the east and north-east. Their surface was more or less bestrewed with small earthy particles, which gave them that dirty grey appearance which old snow always presents at inferior heights, and in positions which are overlooked by more elevated ground. The surface was, moreover, furrowed, and slightly hollowed out, owing to the effects of the wind, and the run of water produced by the partial thaw on the surface, which was much promoted by the great absorption of heat by the earthy particles. Here and there spots were remarked, of a rosy hue, or of the colour of very pale blood, whose form and extent were indeterminate, but which were most conspicuous in the furrows and hollow places. Old snow being always more or less coarsely granular in its nature, we observed that the colouring matter was contained in the intervals between the particles, and this gave to the surface, when viewed near, somewhat of a veined appearance. The coloured spots descended beneath the surface of the snow to the depth of several inches, and often almost a foot; sometimes the colour was most conspicuous on the surface, but at other times it was most remarkable some inches below it. Wherever rocks or stones had occasioned little wells in the snow, the perpendicular sides of these wells were also coloured to the depth of many feet. On the whole, however, the colouring matter penetrated only to a very trifling extent into the substance of the snow, which became more and more compact in proportion to its distance from the surface.

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[*] *Bibliothèque* Universelle de Genève, No. 50. Fevrier 1840.

A sufficient quantity of this coloured snow having been collected and placed in vessels of earthenware, that it might melt, I impatiently waited for the time when I might subject it to the examination of the microscope. As the snow melted, the colouring matter gradually deposited upon the sides and bottom of the vessels, under the form of a deep red powder; a fact which alone rendered the existence of a gelatinous matter very improbable. After two or three hours, the snow being partially melted, I introduced a portion under a microscope of the power of 300 diameters. Expecting to see nothing more than inanimate globules of *Protococcus*, I was not a little astonished to find, that the colouring matter was composed of organised bodies of different forms and natures, some of which were vegetables, but by much the larger proportion, endowed with swift movements, belonged to the animal kingdom. The colour of the greater number was a bright red, approaching sometimes to a blood colour, at other times to crimson, or a very deep brown and almost opaque red. Besides these coloured bodies, there were others not less organized, which were colourless or greyish, the largest of which were evidently of an animal nature, but whose number was so small, that I suspected that their presence was accidental; and, moreover, there was an immense number of very minute spherical bodies, and colourless, evidently of a vegetable nature, which filled up all the spaces unoccupied by the others.

As the infusoria were much more numerous than the algæ, I shall with them begin my description of the organized substances which formed the red snow.

1*st*, The bodies by much the most striking, and which, from their great number, and deep colour, mainly produced the red tint of the snow, were small infusoria of an oval form, whose colour was a very deep reddish-brown, and which were nearly opaque. Measured by the micrometer, their greatest diameter was about $\frac{1}{50}$ of a millimetre, and their smallest about $\frac{1}{150}$. (Fig. 3. pl. 1.) They traversed the field of vision with astonishing rapidity, and in all directions. Although the majority were perfectly oval, with rounded extremities, there were some pear-shaped—that is to say, having one of their ends rounded and obtuse, whilst the other gradually became more slender towards a point, where it was apparently obliquely truncated. The motion of the former was horizontally progressive, whilst the others often stopped in the middle of their course, and for an instant revolved rapidly on their pointed extremity without changing their places. In some of the infusoria of the oval form, I observed, near one of the extremities, or the centre, two small oval spots, which were reddish and almost transparent, and which, after the example of M. Ehrenberg, I regarded as stomachs. Besides this, I could discover no other sign of organization ; and on returning home, where I had an opportunity of consulting the great work on the Infusoria, I have not hesitated to regard them as a yet undescribed species of Ehrenberg's genus *Astasia,* for which I propose the specific name of *Astasia nivalis.* (See Ehrenb. Infus. p. 101, tab. 7, f. 1.)

2*d*, There were among these infusoria, though in very limited numbers, some bodies which were much larger than those above described, in shape round or oval, of a beautiful blood-red colour, inclining to crimson, and which were, to a considerable extent, transparent, and surrounded with a margin or colourless membrane. Their dimensions varied from $\frac{1}{12}$ to $\frac{1}{50}$ of a millimetre. (Fig. 4. pl. 1.) Although I have not been able to observe any movement, nor the slightest trace of internal organization, I have no doubt that these are infusory animals, and I regard them as constituting a new species of the family of *Volvocians,* and of the genus *Gyges* of MM. Bory and Ehrenberg (Cf.

Ehrenb. l. c. p. 51, tab. ii. fig. 31), to which I shall give the name of *Gyges sanguineus*. I am inclined to think that Greville had these same infusoria under his inspection, possibly the identical species, and that he has figured them in his Scot. Crypt. Flora, vol. iv. pl. 231, fig. 8, and partly figures 5 and 6. If, moreover, I rightly understand the passage in which M. De Candolle describes the red snow which was sent to him by M. Barras from Saint Bernard, it would appear that this celebrated naturalist has also observed these animals; and the same form evidently appears in the coloured drawing which Dr Schmidt took at Grimsel in the year 1827.

3d, I also found under the microscope, a small number of other still more minute bodies, which were perfectly spherical, and of a beautiful blood-red colour, though somewhat transparent. Viewed in certain positions, they exhibited at one of their edges a small cleft or very narrow opening. Their diameter was about $\frac{1}{100}$ of a millimetre. Their movement was progressive, and in circles, and they turned upon their axes at the same time. (See fig. 5. pl. 1.) I cannot decide to which of Ehrenberg's genera of infusoria I ought to ascribe this animal. According to the descriptions of many authors, who assign very different dimensions to the globules of the *Protococcus nivalis*, and on comparing it with the drawing of Dr Schmidt already alluded to, I have no hesitation in affirming that this organized substance has been regarded as the diminutive globule of the *Protococcus*.

4th, Among the other infusoria, I have observed, though very seldom, some bodies perfectly spherical, of a very deep crimson colour, slightly transparent at their edges, and surrounded with a membrane, which was without colour. At one determinate point, towards the edge, the colouring mass exhibited an opening, which was transparent, and almost colourless, in the shape of a half moon, and which communicated with the membraneous border. (See fig. 6. pl. 1.) I have not been able to detect any motion in these bodies, and I do not know to what genus I should refer them, although, like the previous ones, they probably belong to the *volvocian* group.

Besides those infusoria which contributed the colouring tint to the red snow, there were some others which were destitute of colour, or greyish. As I have observed these very rarely, it is *possible they were* accidental in the specimen. The first of these was, (5*th*) an infusoria of an oval form, colourless and transparent, enclosing, near one of the extremities, a greyish granular mass. Its greatest diameter was about $\frac{1}{8}$ of a millimetre, its smallest about $\frac{1}{20}$. (See fig. 7. pl. 1.) 6*th*, Some other smaller bodies were spherical, or slightly oval, without colour, transparent at their edge, likewise containing an indistinctly granular, greyish mass, and possessing a diameter of about $\frac{1}{100}$ of a millimetre. (See fig. 8. pl. 1.) This form has a striking resemblance to the *Pandorina hyalina* of Ehrenberg (l. c. p. 54. tab. ii. fig. 34.). 7*th*, Finally, I have observed a single individual, colourless and transparent, apparently composed of two spherical globules, united together without any trace of contents, or any organization whatever. The diameter of one of these globules might be about $\frac{1}{200}$ of a millimetre, or somewhat more. (See fig. 9. pl. 1.) It is possible that this form should be referred to the *Monas gliscens* of Ehrenberg (l. c. p. 13, tab. i. fig. 14.). In these three colourless infusoria, I cannot affirm that I could discover any motion.

After having thus described, as accurately as I can, the organic forms which I believe belong to the animal kingdom, I now proceed to describe the true algæ of the red snow, and another devoid of colour, which is to be found in many other situations, and which, in my apprehension, has given rise to numerous errors in the description of the *Protococcus nivalis*.

8*th*, I have observed in small number, but without fail, upon all opportunities, some spherical globules of a brilliant blood-red colour, evidently full of a granular mass, and consequently possessing an imperfect degree of transparency. They had all nearly the same dimensions, their diameter being from $\frac{9}{300}$ to $\frac{1}{30}$ of a millimetre. (See fig. 2. *a*. pl. 1.) I have never detected in them either a gelatinous matrix, or a membranous border, or any movement whatever. When bruised, they allow their colouring matter to escape under the form of infinitely small granules, which are very numerous, and the membrane remains torn and colourless. The same effect was produced by the evaporation of all the water which was under the microscope. (Fig. 2. *b*. pl. 1.) This was the *Protococcus nivalis* of Agardh, who did not perceive the contained granules, on account of his not employing a sufficiently powerful microscope.

9*th*, In the midst, and around all the bodies already alluded to, whether animal or vegetable, there was an incalculable multitude of very small spherical globules, which were colourless, detached, or united in groups, without any trace of motion, or any contents whatsoever. Their diameter is not more than the $\frac{1}{500}$ of a millimetre. (See fig. 10. pl. 1.) On isolating one of the larger bodies from the rest, a considerable quantity of these minute globules collected round it, and often assumed a filamentous, articulated, or cellular appearance. (See fig. 2. *c*. pl. 1.) In proportion as the water, contained between the two plates of glass, evaporated, the same effect continued to be produced; the primitive structure becoming indistinguishable; if again moistened, these bodies reassumed their appearance very imperfectly. This was the *Protococcus nebulosus* of Kutzing. (Linnea 1833, p. 365, tab. 3. fig. 21.) I have no doubt that it is to this organism we should refer the minute colourless globules observed by Bauer, and others which float upon the surface of the water. No more do I doubt that, in many cases, it is these small globules, become obscure by the effect of drying and decomposition, and mixed with the colourless remains of the globules of the *Protococcus nivalis*, which have led many naturalists to believe in the necessary pre-existence of a matrix or gelatinous substratum.

I ought to remark that it was about 4 P.M., and in cloudy weather, that I made the preceding observations, and that coming darkness obliged me to delay taking drawings till the morning. Even at 11 P.M., the snow contained in the vessels was not quite melted. At an early hour, however, next morning, it was so; and the colouring matter was deposited on the bottom of the vessels; and I perceived by the microscope, that all life had terminated in the different objects; and the globules of the *Protococcus* could not be distinguished from the infusoria of the fifth figure of the plate, except by their colour, which was brighter, by their great transparency, and their contents being evidently granular. The figures, therefore, were made from individuals who had been for some time dead.

In the hope that the weather would improve, and that the red snow would continue to grow, I delayed next day going again in quest of it; but, in the evening, the weather was worse, and next day fresh snow descended in great flakes.

Having prepared for my herbarium, upon mica, a number of specimens of the colouring matter, the remainder was put into a concave watch-glass, and there allowed to dry. Protected against the action of light by being wrapped in white post paper, the colour did not change; and when moisture was added, the globules of the *Protococcus nivalis* and the *Astasia nivalis* might still be distinctly recognised. I found also the globules of the *Protococcus nebulosus* under the form of filamentous debris; but all the other organized bodies could not be distinguished, at least with any certainty. The specimens upon the mica exhibit a somewhat singular phenomenon; for, having occasion subsequently to examine them, I found that two kinds of paper having been employed in covering them, the one yellowish, and the other bluish, the red snow had lost nothing of its colour in those in the former kind of paper; whilst, in the others, all that remained were colourless or greenish globules; a result probably owing to the chemical composition of the different kinds of papers.

The existence of this remarkable fact, which I believe has not hitherto ever been suspected, viz., that, in the red snow, there exists an infinite number of microscopic beings, which are evidently animals, and at a temperature rarely elevated more than a few degrees above the freezing point, and often probably far below it, shews how much yet remains to be discovered in this new world, the limits of which will be extended in proportion as our microscopes become more perfect. Unfortunately the great work of Ehrenberg, indispensable for all who would prosecute the study of these organisms, is, from its form and cost, inaccessible to many naturalists,—a circumstance which will much retard the progress of this branch of science.

The coexistence of these infusoria along with the *Protococcus nivalis*, the contents of which appear to supply them with nourishment, will, I trust, destroy, if it really exist, the opinion of those who still maintain the theory of the transformation of vegetable into animal organisms; a theory which, in my opinion, is based upon false appearances, and which ought never to have been broached, being wholly opposed to every thing like true philosophy.

The extreme sensibility of these infusoria to the action of heat, by which they are destroyed at a temperature a few degrees above that of the surface of the snow, and perhaps also an incapacity of supporting all displacement and jolting, are probably the causes why their presence, as in part a cause of red snow, has hitherto remained unknown. On the other hand, I do not mean to maintain, that the infusoria above described are always to be found bearing so large a portion of the colouring matter of the red snow (in my observations the globules of the *Protococcus nivalis* were to the infusoria in about the proportion of 5 or 10 to 1000); on the contrary, it appears to me probable, that the number of the globules of the *Protococcus* often surpasses that of the infusoria; for, in an analogous production, viz. the *Hæmatococcus Noltii*, Ag. (discovered in the year 1838, by Dr Smith, in the water of the turf ditches, *fosses de la tourbière*, of Gumlingen, near Berne), with which a species of the *Astasia* was found intermixed, and also the *Protococcus nebulosus*, the relative quantities of the two productions presented considerable differences; sometimes the *Astasia* was found there almost to the exclusion of the *Hæmatoccocus*, and sometimes the granules of this latter predominated considerably.

In comparing my observations with those of others, it appears evident that Bauer especially, and Unger, have described as a gelatinous matrix the colourless remains of the *Protococcus nivalis* and *nebulosus*; for, so far as our Alps at least are concerned, the general distribution of the colouring matter in the substance of the snow, to considerable depths, and its gradual deposition upon the sides and bottom of the vessels, in proportion as the snow melts, demonstrate that it can have no substratum whatever in the fresh state.

As to the reproduction of flakes of this same gelatinous and filamentous matrix, and the fresh development of new colourless organised bodies, observed by Bauer, I doubt not that the organisms were on these occasions altogether new, and quite independent of the red snow. For all observers, however little they may have been engaged with the study of microscopic organisms, whether vegetable or animal, must know with what wonderful celerity the species of *Hygrocrocis*, *Protococcus*, &c. on the one side, and the *Chenas*, and other infusoria, on the other, develope themselves; so much so, that I believe the *Protococcus nebulosus* might readily develope itself *during the short time* the snow remained in the vessels where it was melted, without having previously co-existed with the other organised bodies of the red snow.

Although the celebrated Algologue Agardh has stated that the *Lepraria kermesina* of Wrangel is the same production with his *Protococcus nivalis*, yet this identity I consider to be still doubtful. It is possible; but I confess I am inclined to believe, that we shall succeed in demonstrating that the production found hitherto only upon stones, and the decomposed remains of other plants, is a quite distinct organism. After what I have already said, I need not dwell upon the *animality* of the globules observed by Wrangel, and upon the supposed transition of vegetable globules into infusoria, and *vice versa*.

The Scottish plant figured by Dr Greville (which had previously been noted by Agardh as belonging to a distinct genus), and probably even the plant discovered upon the stones and mosses of the polar regions, and described by Hooker, under the name of *Palmella nivalis*, appear to me not only distinct species, but, if the existence of the gelatinous substratum observed by Hooker in the polar plant, and so distinctly figured by Greville, both in it and the other, be verified, then these plants (notwithstanding Mr Harvey's remarks), cannot be confounded with the *Protococcus nivalis*, any more than they can even enter into the genus *Hæmatococcus*. The plant observed upon the stones, &c. near Prague, and figured by Corda, had been before noted by Kutzing (in the Linnea 1833, p. 372), as his *Microcyrtis sanguinea* (*Hæmatococcus sanguineus*, Ag. Ic. Alg. Eur. No. et Feb. 24.), and apparently correctly.

Upon the whole, then, it appears neecssary to distinguish the different Algæ which have been confounded under the name of *Protococcus nivalis;* and as, according to my observations, the diagnoses of the genera appear to me no longer satisfactory, I shall now endeavour to propose others, beginning with the most simple organization.

Protococcus, Agardh, Syst. Alg. p. xvii. Globuli liberi sporulis repleti. *Protococcus nivalis*, Ag. l. c. p. 13. Icon. Alg. Eur. No. et tab. 21. *Pr. nivalis*, tabula nostra, f. 2. *Uredo nivalis*, Bauer, l. c. Nees ab Esenb. in Brown's Verm. Schrift. i. p. 578, cum icone excl. f. 9.

The character of this genus will exclude, so far as we actually know, a great portion of the other species which are usually introduced into it, as the *Protococcus nebulosus*, Kutz. l. c. and fig. 10 of our plate; but I have no doubt that more powerful magnifiers will enable us to perceive internal sporules.

Hæmatococcus, Agardh, Ic. Alg. Eur. No. et tab. 22 et 24. Globuli liberi sporidia sporulis repleta includentes. *Hæmatococcus sanguineus*, Ag. l. c. No. et tab. 24. *Microcyrtis sanguinea*, Kutz. in Linn. 1833, p. 372. *Protococcus nivalis*, Corda in Strum D. Fl. et Kutz.

The Scottish plant figured and described by Greville, is also placed in this genus by Agardh, under the name of *Hæmatococcus Grevillii*, on account of the large granules which it contains. These granules, judging from the *Hæmatococcus Noltii* already mentioned, which I have examined in the fresh state, should be sporidia, that is to say, not *sporules*, but *thecæ*, in which the true sporules are contained, as in the genus *Hæmatococcus*, such as I have defined it. But the presence of a gelatinous substratum, as to the accuracy of which (in virtue of the confidence I place in the observations of my friend Dr Greville) I am inclined not to entertain a doubt, must naturally exclude it from this genus, and place it in a higher one of the system. Approximating closely to the *Palmella*, it will be distinguished from this genus principally because the globules are external, and not inclosed in the gelatine. For this genus, then, I shall propose the name of—

Gloiococcus, Shuttl. Globuli massæ gelatinosæ affixi, sessiles, sporidia sporulis repleta includentes. *Gloiococcus Grevillii*, Shuttl. *Protococcus nivalis*, Grev. Scot. Crypt. flor. No. et tab. 231. excl. syn. *Hæmatococcus Grevillii*, Ag. Icon. Alg. Eur. No. et tab. 23. *Microcyrtis Grevillii*, Kutz. Linn. 1833, p. 372.

I do not know if the 9th figure of Bauer's plate belongs to this last description, but it is the more probable, in that Harvey regards the plant of the polar regions as identical with the Scottish, and I am inclined to believe that the *Palmella nivalis* of Hooker (l. c.) is probably to be referred to this group.

Before concluding, I must remind my readers that nothing is so uncertain as observations made on microscopic organisms which have been dried for the herbarium, or which are dead. I hope, therefore, at some future period, to be able to communicate a more detailed account of the organization of the infusoria of red snow, upon my obtaining an opportunity of examining this remarkable production under more favourable circumstances.

EXPLANATION OF THE PLATE.

Fig. 1. A portion of the colouring matter of the red snow.

Fig. 2. *a*, Globules of the *Protococcus nivalis* Ag.

b, A globule whence the sporules are escaping.

c, A globule surrounded with globules of the *Protococcus nebulosus* of Kutz.

Fig. 3. Astasia nivalis, Shuttl.

Fig. 4. Gyges sanguineus, Shuttl.

Fig. 5. } Fig. 6. } Other coloured infusoria of red snow.

Fig. 7. } Fig. 8. } Other uncoloured do.

Fig. 9. Monas gliscens, Ehrenb.?

Fig. 10. Protococcus nebulosus, Kutz.

It is to be observed that all these figures were examined through a microscope which magnified 300 diameters.

Report on a Memoir entitled "*Observations on the Marine, Land, and River Mollusca of the Sechelles and Amirantes Islands.* By M. Dufo." By M. Blainville.

The knowledge of animals does not consist merely in an acquaintance with their external and internal organization, their specific distinction, and their position in the natural series. The consideration of these may be said to be reserved for professed zoologists, because, in order to do it aright, preliminary study is requisite, the inspection of collections, books of description, and especially of iconography. But the knowledge of the manners and habits of animals is equally required, things which, although often the evident deduction from peculiarities of organization, are not always certainly ascertained from that source. The study therefore, of living animals in their natural haunts, their relations with the soil, with the medium in which they live, with the other organized bodies on which they subsist, or to which they afford nourishment, and their mode of propagation, is of so much importance, that, in the common opinion and language, it seems to form the whole science.

It is to this important part of our knowledge of the molluscous animals, or to their natural history, that the labours of M. Dufo are calculated to add a considerable number of new facts, which will be doubly augmented in value when we reflect that, in order to acquire them, it was necessary to have the energy to reside for many years in places purposely selected on account of the great abundance of molluscous animals which they afford; and the places most favourable for the multiplication of these animals, be it remarked, are commonly most remote from human habitations, especially from the habitations of civilized man; and this circumstance renders great devotion to the subject, and also considerable expense, requisite, in order to obtain the opportunities most favourable to observation.

With this object in view, M. Dufo, induced by his love for conchology more than any thing else, and with no other mission than the desire to be useful, took up his residence among the Sechelles and Amirantes Islands, which present a great number of creeks and rocks, sandy flats and shoals, which are but rarely visited by navigators. Taking along with him a few negroes and suitable provisions, he could here deliver himself up, without fear of interruption, to long and repeated observations on many points in the natural history of the conchyliferous mollusca, and among others the *operculum*, and the different modifications through which shells pass from youth to their decline.

The first point, so long neglected, to such a degree, indeed, that the study of it may be said to have commenced in our days, and, in the works of one of the present reporters, has acquired great importance since it has been demonstrated that it may be employed not only for the distinction of species of which we possess only the shell, but for confirming truly natural generic groups. M. Dufo has established, in regard to a great number of species of the genera Fusus, Turbinella, Murex, Purpura, Buccinum, &c. that this view of the matter is correct. Thus, he has shewn by the operculum of the so-called *Buccinum undosum*, that it is a species of the genus *Turbinella*, and by that of *Cerithium palustre*, which differs, by the composition of the imbricated circular elements, from that of the true Cerithiums, that it does not belong to this genus. In like manner, he has confirmed the genus Potamides, established by M. Alexander Brongniart, for the fossil shells regarded before his time as Cerithiums, and which the fluviatile nature of the locality in which they are found, as well as some peculiarities in the form of the opening, had caused to be separated from the Cerithiums, which are marine. In truth, *C. palustre*, as the name indicates, inhabits the fresh waters of marshes.

Science will likewise be indebted to M. Dufo for a positive knowledge of the fact, that the operculum is wanting in the genus *Terebellum* (*Tarière*), which formerly rested on supposition merely.

The second point on which M. Dufo's observations have most essentially borne, is that of the successive forms through which shells pass, from the earliest age of the animal which bears them, till its decay. This is a point of extreme importance, and evidently connected with the fact of the diminution of the lobes of the mantle with age, as M. Dufo has again confirmed.

In truth, from the time that geology, while passing into the state of a science, acquired, in the organized bodies whose fossil remains exist in the superficial strata of the earth, one of the most powerful means for solving the problems of the identity or antiquity, or even the origin of these strata, the study of shells, which, from their chemical nature, may form extensive rocks, has acquired a very great importance. But, unfortunately, since M. Lamarck, so justly celebrated, has regulated fossil conchology by the distinction and naming of the species, many geologists, who are often but little acquainted with natural history, have seized upon this part of the science; and then, sometimes constrained rather by the wants of geology than enlightened by a real knowledge of zoology, they have established and named as species a great number of fossil shells, without taking into account the limits of variation which those parts of molluscous animals undergo, and, in fact, before malacology was at all in a condition to meet the wants of science. One of ourselves, during the few years he occupied the place of M. Lamarck in the Museum of Natural History, having felt how important it was to examine the limits of variation, before adducing laws, had begun to form series of shells of the same species, having a regard not only to the age, but likewise to the sexes of the dioecious kinds, as well as to their localities. Guided by these attempts, M. Dufo has gone much farther. In the collection of shells brought by this zealous observer, we observe series of a considerable number of species, the variations of each of which amount to upwards of fifty; and the variations affect not only the size, but all the distinctive peculiarities which shells can present. In this respect, M. Dufo's collection is of great interest, particularly in the genera Purpura, Ricinula, Turbinella, Murex, Cypræa, Strombus, and Pterocera, since it enables us to appreciate the limits of variation of which one species of shell is susceptible, even in climateric and other circumstances absolutely alike. To what would this lead, if to these were added the varieties which the same species exhibits when living in more or less distant localities?

Besides these two important points in malacology, M. Dufo has directed his attention to many others not less interesting, and has filled up some blanks in the natural history of the mollusca.

Thus the depth and nature of the parts of the sea which different mollusca prefer, have been carefully noted by M. Dufo. He has remarked, for example, that the bivalves which live in the sand bury themselves deeper as they advance in age; that certain species of Cerithium live solitarily, and others in society.

He has likewise paid attention to the kind of nourishment preferred by each species; and if in this respect M. Dufo has confirmed in a great measure Lamarck's division of the Trachelipods into zoophagous and phytophagous, he has likewise removed some of the errors of that skilful zoologist. Thus, according to him, the Cerithiums are exclusively phytophagous, as well as the Cones and Cypræas, contrary to what Lamarck supposed.

Finally, it is not the same with regard to the mode and celerity of locomotion in a considerable number of species observed by M. Dufo. Thus the strombi and pteroceræ advance by what may be called successive bounds; the cones are very inactive, while it is otherwise with the Cypræas, as might have been anticipated from the great difference in the extent of their locomotive disc.

The considerable period (four years) during which M. Dufo continued his observations, enabled him to judge of the duration of the life of some of the species by the slowness of their development. This presumption refers more particularly to *Cerithium palustre*.

Among the peculiarities which it would be difficult to class under any of the above heads, we may mention the following :

Buccinum arcularia has its operculum finely denticulated round the edges, and this it seems inclined to use as a defence when one is about to seize it.

The double foot of the Harpæ (first noticed by M. Quoy, to whom the science is indebted for a great number of new facts in malacology), which appears to replace the operculum, wanting in this genus, falls and breaks on the slightest effort, and thus seems to afford the animal a means of escape from the voracity of its enemies, by leaving to them this part of its body.

In the Cypræas, the lobes of the mantle are in a singular state of continual vibration, which does not take place in those of the Ovula, a genus so nearly allied to the Cypræa.

The aëriferous vesicles in the foot of the Janthinæ are completely empty when the animal is at a certain depth in the sea.

The *Achatina mauritiana* deposits its eggs in a column forming a train of some length ; but the most remarkable fact of this kind observed by M. Dufo, is, that certain Helices are ovoviviparous, like several species of Littorina, the *Paludina vivipara* of our rivers, the Partulas, &c. : that is to say, that the eggs are evolved in the end of the oviduct, and the young issue from the mother in a living state.

Some species of Calyptræa are provided with a support distinct from the rock on which the animal rests, while among the living Hipponices, the support forms part of the rock, and is hollowed on the surface.

Finally, M. Dufo seems to have ascertained that certain bivalves, provided with a byssus, detach their byssus piece by piece, as was previously conjectured.

While limiting ourselves to this simple enumeration of the principal facts determined by M. Dufo, we may be permitted to add, that if, among the truly immense number of shells which M. Dufo has brought with him, we find only forty or fifty new species,—which are both more easily obtained, and more highly valued for collections,—he has not failed to furnish us with information regarding species already known which will greatly advance their history, and which was much more difficult to be acquired. To attain the latter object, much more is necessary than to collect these animals, and to put them among a liquid fitted for preserving them, as is almost always done by travelling naturalists. It is necessary to pass days, months, and years in observing the animals, and carefully noting all their peculiarities. M. Dufo's work must be admitted not to be essentially scientific; to that the author makes no pretension: but his researches are of real importance, first, in themselves, and, secondly, in their rarity; and they will tend not a little at once to enrich the works of naturalists, and the collections of our museums.

We think it our duty to propose to the Academy, that its thanks should be given to M. Dufo for the zeal with which he has fulfilled his self-imposed mission; and to invite him to continue it, if he can; and, in that case, to turn his attention to the animals themselves in their relations to the shell,—to the differences of sex, and to the eggs of each species, matters hitherto but very little known in the history of molluscous animals, and which will exercise a great influence on the ulterior progress of the science.

The conclusions of this report were adopted by the Academy. The commissioners were MM. Dumeril, Milne Edwards, and Blainville.—*Comptes Rendus*, N. 10. March 1840, p. 392.

On the Hair in Man, and the Dermal Coverings of Animals.

During several of the late meetings of *La Société Philomatique* of Paris, there have been animated discussions concerning the growth of the hair, and some of the phenomena connected therewith, a summary of which we shall now endeavour to submit to the notice of our readers. These discussions have originated in the recent publication of M. Mandl "on the Hair and other Tegumentary Coverings," and the learned author has borne his share in these disputations. One of the propositions which M. Mandl is disposed to maintain, is, that the hair, and other dermal coverings, shoot up or vegetate, not from the bulb or root only, but grow also from the point. In proof of this, at the meeting of the 22d of February last, he particularly insisted upon the result observed after some careful experiments, viz., that although hair cut clean across presents at first a completely truncated extremity, yet at the end of several weeks it acquires the form of a fine point. Hence he contends that there is a movement of the nutritious juices in the interior of the canal of each hair; he is at the same time of opinion, that the growth of the bulb is carried on by a kind of intus-susceptio. In opposition to this view, M. Huzard remarked, that his father having made some experiments on the effects of dyes applied to the wool of living animals, was led also to make observations upon the growth of the wool; and he found that after having dyed the wool of one year's growth with one colour, and tinged that of a second year with another, that at the end of the third he could not perceive that the spaces marking the growths of the previous years had increased in the slightest degree,—whence it followed that the wool shot out only at its base.

Another point discussed, was the cause of the hair becoming white; and one or two additional facts, bearing on the point, were mentioned. M. Roulin stated, that he was acquainted with an individual, who, being terrified at the prospect of losing his fortune, in one night the hair on one-half of his head became white, the half being the one on which he had reposed. M. Roulin also stated another very striking fact, quoted from the London Magazine, viz., that a cock which had been captured by a fox, and rescued again in life, speedily lost all his feathers, which, however, grew again, but all of a white colour. The true cause of hair becoming white, in the estimation of this gentleman, was the disappearance of the colouring oily fluid, which generally fills the tubes of the hair. This explanation, however, in the opinion of M. Doyère, was in no degree satisfactory, inasmuch as even although the absence of this colouring fluid were conceded, yet this would not render the hair white but only transparent, and it is well known that hoary hair is not only white but opaque also.

But, by far the most interesting opinion elicited during these debates was, that hoary locks, sometimes again become black, and that there was a process by which the change could be artificially induced, and whereby red and light coloured hair could be made black. Several statements were made, whence it would appear, that the Chinese have long been familiarly acquainted with this art. One instance of which we shall detail. M. L'Abbe Imbert, now favourably known as having given an account of the manner in which the Chinese make their artesian wells, or spouting fountains, came to Paris in the year 1823, to make preparations for his mission to China. At that time his hair was of a glaring red colour; on arriving, however, at his destination, those interested in his success, to prevent his immediate detection as a stranger and foreigner, amidst a people universally black-haired, supplied him with a secret retreat, and subjected him to a constitutional and internal treatment which speedily turned into black the hair over his whole body; in which state he was seen by L'Abbe Voisin and many others. This extraordinary transformation being effected on the carroty locks of M. Imbert, we are the more prepared to admit its possibility in hair that has become blanched. M. Roulin accordingly mentioned a fact which was communicated to him by L'Abbe Voisin a missionary, who had long resided in China, and who was himself the subject of the transformation. On his arrival in the Celestial Empire, his locks had already become grey, and before he was allowed to hold intercourse with the inhabitants, "he was subjected to a treatment, consisting of internal remedies only, the result of which was to blacken his hair not temporarily only but permanently." M. Guerin also, in confirmation, stated, that he was acquainted with two missionaries, who had hoary locks when they set off for China, and who, on their return, had hair perfectly black. He understood that the remedy, as it is called, for the production of this effect, consists in an infusion of three kinds of plants, followed up by a peculiar regimen. Several of the members, notwithstanding the evidence adduced, could not overcome their incredulity in the matter; and M. Velpeau adduced the case of M. Rochoux, whose hair at one time was white, whilst now it is as decidedly black. In his case, it was not the result of any remedy or any course of treatment. M. Roulin, to shew that the agents used might have the effect attributed to them, quoted the cases of several individuals who had laboured under indisposition, and who had observed a change of the colour of their hair, under the treatment to which they had been subjected; whilst finally it was cited that the bright feathers of the bullfinch become quite black if the little songster be subjected to the long continued use of hemp-seed alone as food.

On the Lake of Zirknitz in Carniola.

In the fifth volume of the "*Zeitschrift für Physik*," M. Leander Knöpfer gives a description of the Lake of Zirknitz as he found it during a visit in 1837. He says, that when he obtained a view of the village of Zirknitz, and the small towns lying around it in the plain, he looked in vain for a sheet of water resembling a lake; he could only see on the opposite hills a longitudinal white stripe, which at a distance had the aspect of a sandy *steppe.* This was in fact the deep bed of the lake, in which the small quantity of water remaining behind flowed in separate large channels like artificially formed canals (which rendered impossible an ascent for any great distance), towards several larger openings, into which it fell with a rumbling noise. Two of these breaches were distinguished by their size and considerable depth. Several, perhaps all, of these passages for water, might soon unite, in their subterranean course, into one and the same canal, or might soon again separate, according as the power of the water could, by its natural pressure, form passages in the weathered and perforated beds of limestone. Eventually the water again makes its appearance in Freudenthal, near Ober-Laibach, from copious springs, and forms, by being united in a channel, the river Laibach, which, with exception of the time during which the lake is dry, is navigable at its very source.

As the whole bottom of the valley in which the lake lies is so shut in all round by mountains, which are branches of the Julian Alps, that the water flowing together can find an exit at no lower point in it, Nature, as it has likewise done in many other places where the same relations exist, formed here a large deep lake, which would only have found an exit at a height of several hundred feet on the north-eastern side, had it not been for the loose and perforated ground which admitted of a subterranean passage for the water. When the mass of water flowing into it is smaller, as in the height of summer, and when, consequently, the subterranean flowing off is greater than the amount collected, the lake falls, like a pond which has been artificially emptied, and so much the more rapidly in proportion to the smallness of the water poured in. If at this time there should be violent storms or continued rain, the sinking of the water of the lake ceases, and the level of the water either remains for some time without a perceptible difference, or it even becomes higher, and anew fills the whole basin. It must hence be evident, that no fixed time can be assigned, at which the water of the lake flows away, or again fills its basin. It can as easily be understood, that the water forcing itself through the subterranean canals, should more and more enlarge the cavities in the soft limestone; should separate smaller or larger portions from the walls of these cavities, and carry them away; and should thus, at narrow places, interrupt the channels for a period. At present, the water of the lake, when the sinking begins, flows away in a much shorter time than it did 150 years ago, and it likewise takes a longer time than at that period to fill the lake. We may, therefore, conclude with great probability, nay, almost with certainty, that, in the course of time, though probably not for centuries, the Lake of Zirknitz will altogether cease to exist.

The author has the praiseworthy desire to correct the wonderful narrations which occur in the older works by Sartori, Valvassor, &c., and which have been, within a few years, repeated by some authors; and especially to contradict the statement that the ebb and flow of the lake are somewhat periodical—that they are in some degree regulated by the day and hour, and that the water makes its appearance from the same apertures by which it flowed off. With this object in view, he adds the following information. The Carthusians of Freudenthal, to whom the Princes of Eggenberg, at that time lords of Zirknitz, had, towards the end of the seventeenth century, relinquished and ceded the right of fishing in the lake, knew well how to employ for their advantage what has just been said regarding the lake and its flowing off. Their strict rules forbade them to partake of a meat diet, and hence a good take of fish at all times of the year was of great consequence to them. The drying up of the lake was thus particularly inconvenient; and they, therefore, endeavoured to prevent it, by covering, when the basin was empty, all the openings with iron grates, then placing on these several slabs of stone secured by means of clay, and filling up the cavities with earth. In this manner they often succeeded in actually keeping the lake at its high level for years together, which could not have been possible had Valvassor's view been accurate. The Monks sought out most indefatigably all apertures which presented themselves from time to time, in order to stop them up anew, until the dissolution of the fraternity left the lake to its fate.

The opinion now given is, undoubtedly, the correct one, and ought to be made generally known. But it ought likewise to be remarked, that Tobias Gruber, who visited the Lake of Zirknitz in April 1773, expressed essentially the same view in his "*Briefe Hydrographischen und Physikalischen Inhalts aus Krain*," published at Vienna in 1781. His full description of the lake, which is illustrated by plates, coincides perfectly with that given by M. Knöpfer. According to him, it is, more particularly, two large caverns at the foot of the mountain *Javornik* (*Jauernik*), called *Vranja Jama*, and *Sucha Dulza* (*Seka Dulka*), from which the water, when much rain has fallen on the mountains, or much snow has been melted there, rushes out with great violence and hastens to the lake; and in the same manner, according to the same author, it again flows away through many small cavities, but especially through two passages at the east end of the lake, the larger and smaller *Karlauza* (*Mala* and *Velka Karlouza*). But Gruber likewise only saw the lake at the period of its ebb; and it were much to be wished that a naturalist should be a witness of its flow.—(*Poggendorff's Annalen*, 1840.)

Notes on some rare Scottish Minerals. By Professor L. A. NECKER of Geneva. Communicated by the Author.

1. *Crystallized hydrate of magnesia* occurs at Swinaness, in the island of Unst. The crystals are small hexagonal tables, which are parallel to the lamellæ of the lamellar specimen of hydrate of magnesia, of which they form part. These are very short six-sided prisms, having their three alternate terminal edges truncated by planes which are oblique to the axes, thus seeming to indicate a rhomboid as the primitive form of this mineral. I did not find the specimen myself, as the vein is now exhausted from which this substance was formerly obtained for collections, but I procured it from a native of the neighbourhood of Swinaness, who had collected it. On one of the small crystals, there are feeble traces of an oblique face on the terminal edges, which are not modified in the others.

2. *Arragonite crystallized in simple forms* occurs, lining the walls of a small fissure, in the serpentine of Swinaness. The crystals are rhomboido-prismatic, are terminated by diëdral summits (sometimes by four-sided pyramids ?), and are placed on their matrix parallel to one of their lateral edges, or to one of the sides of the prism. They are accompanied and covered by a yellow translucent matter, which seems to me to present passages to the white aragonite, and which effervesces with acids. Is it not a ferruginous aragonite, or the junckerite of Dufrenoy ? The prisms of aragonite of Swinaness have six or eight sides.

3. *Gallinace of Beal in Skye.*—This massive mineral, or rather this apparently homogeneous rock, has been taken for a retinite or pitchstone by Messrs Murchison and Sedgwick, in their excellent description of this part of Skye; and nothing, indeed, can bear a greater resemblance to pitchstone than this substance, but it differs essentially from it in its specific gravity, and by its fusion before the blowpipe into a black scoriaceous enamel. It forms a crust or rind of two or three inches in thickness, on a basaltic dyke, which traverses the whole valley of Beal, like a prominent wall composed of horizontal prisms. Probably the vitreous crusts of the dykes of

Carsaig, in the island of Mull, and of Lamlash, are also of *Gallinace*. The *Gallinaces* present the same relations to the basalts, as the pitchstones do to the trappean and felspathic porphyries, the vitreous lavas to the compact, and the obsidians to the trachytes. The *gallinaces* are vitreous basalts, while the pitchstones are vitreous felspathic traps.

4. *Green diopside* is found in the upper part of the valley of Beal. It is of an impure green tint, is very translucent, presents a form analogous to that of augite, and replaces the common augite in a large grained basalt, being often mixed with a bluish calcedony.

5. *Levyne*, in small crystals, similar to the form figured in Allan's edition of Phillips' Mineralogy, of a milk-white colour, and semi-translucent or opaque, occurs in cavities in the beautiful basaltic amygdaloid of the Storr in Skye, and in the whole basaltic northern portion of the island; also at Quiring. In both these localities, the chabasite and levyne are always separate. I have only seen one or two cases where isolated crystals of chabasite were found in the cavities occupied by the levyne, and I have never seen the levyne in those of chabasite.

6. *Lumachella marble*, analogous to that of Bleyberg in Carinthia, is met with at Loch Shiant, on the west side of Loch Staffin, in Skye. The pieces of ammonite, of a high lustre and brilliant colour, which give it the peculiar character, are of rare occurrence, and the bed in which they are met with is below high water-mark.

7. Very large portions of mica are found in Glen Shiel, near the road leading from Glenelg to Fort Augustus, and about half-way between the inns of Shiel House and Cluny. The plates are nine inches in length by six in breadth, and three-fourths of an inch in thickness; but, as I did not procure them myself, I cannot say if they are found in the gneiss itself, or in granite veins. There are on the surfaces very distinct traces of cleavage, forming striæ arranged in equilateral triangles, as in specular iron-ore; a circumstance which would have led me to believe it to be uno-axial or rhomboidal, but an experiment made along with Professor Forbes proves it to be diaxial or prismatic.

8. *Garnets.*—The talc or chlorite slate of the Glen Shant

rock, at the lower extremity of Glen Rosa in Arran, has afforded me brown and yellow grossulaire garnets, red translucent almandine garnets, and massive colophonite garnet.

7. Blocks of very fine granular granite, near Brodick, include very minute bluish-*green beryls*,* and white and honey yellow *topazes*.

10. *Red ferruginous augite* or pyroxene occurs in large blocks of basalt, at the northern entrance of the village of Corrie, in the island of Arran.

Account of the Capture and Death of a large Alligator, at Manilla, in the Island of Luconia, one of the Philippines.

In the course of the year 1831, the proprietor of Halahala, at Manilla, in the island of Luconia, informed me that he frequently lost horses and cows on a remote part of his plantation, and that the natives assured him they were taken by an enormous alligator, who frequented one of the streams which run into the lake. Their descriptions were so highly wrought, that they were attributed to the fondness for exaggeration to which the inhabitants of that country are peculiarly addicted, and very little credit was given to their repeated relations.

All doubts as to the existence of the animal were at last dispelled by the destruction of an Indian, who attempted to ford the river on horseback, although entreated to desist by his companions, who crossed at a shallow place higher up. He reached the centre of the stream, and was laughing at the others for their prudence, when the alligator came upon him. His teeth encountered the saddle, which he tore from the horse, while the rider tumbled on the other side into the water, and made for the shore. The horse, too terrified to move, stood trembling when the attack was made. The alligator, disregarding him, pursued the man, who safely reached the bank, which he could easily have ascended, but, rendered foolhardy by his escape, he placed himself behind a tree which had fallen partly into the water, and drawing his heavy knife, leaned over the tree, and, on the approach of his enemy, struck him on the nose. The animal repeated his assault, and the Indian his blows, until the former, exasperated at the resistance, rushed on the man, and seizing him by the middle of the body, which was at once enclosed and crushed in his capacious jaws, swam into the lake. His friends hastened to the rescue; but the alligator slowly left the shore, while the poor wretch, writhing and shrieking in his agony, with his knife uplifted in his clasped hands, seemed, as the others expressed it, "held out as a man would carry a torch." His sufferings were not long continued, for the monster sank to the bottom, and soon after reappearing alone on the surface, and calmly basking in the sun, gave to the horror-stricken spectators the fullest confirmation of the death and burial of their comrade.

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* We found in the island of Arran, many years ago, a specimen of graphic granite, containing blue beryls, one of the crystals nearly an inch long.—Edit.

A short time after this event, I made a visit to Halahala, and expressing a strong desire to capture or destroy the alligator, my host readily offered his assistance. The animal had been seen a few days before, with his head and one of his fore feet resting on the bank, and his eyes following the motion of some cows which were grazing near. Our informer likened his appearance to that of a cat watching a mouse, and in the attitude to spring upon his prey, when it should come within his reach.

I would here mention, as a curious fact, that the domestic buffalo, which is almost continually in the water, and, in the heats of mid-day, remains for hours with only his nose above the surface, is never molested by the alligator. All other animals become his victim when they incautiously approach him, and their knowledge of the danger most usually prompts them to resort to shallow places to quench their thirst.

Hearing that the alligator had killed a horse, we proceeded to the place, about five miles from the house. It was a tranquil spot, and one of singular beauty, even in that land. The stream, which, a few hundred feet from the lake, narrowed to a brook, with its green banks fringed with the graceful bamboo, and the alternate glory of glade and forest, spreading far and wide, seemed fitted for other purposes than the familiar haunt of the huge creature that had appropriated it to himself

A few cane huts were situated a short distance from the river, and we procured from them what men they contained, who were ready to assist in freeing themselves from their dangerous neighbour. The terror which he had inspired, especially since the death of their companion, had hitherto prevented them from making an effort to get rid of him, but they gladly availed themselves of our preparations, and, with the usual dependence of their character, were willing to do whatever example should dictate to them. Having reason to believe that the alligator was in the river, we commenced operations by sinking nets, upright, across its mouth, three deep, at intervals of several feet. The nets, which were of great strength, and intended for the capture of the wild buffalo, were fastened to trees on the banks, making a complete fence to the communication with the lake.

My companion and myself placed ourselves with our guns on either side of the stream, while the Indians, with long bamboos, felt for the animal. For some time, he refused to be disturbed, and we began to fear that he was not within our limits, when a spiral motion of the water, under the spot where I was standing, led me to direct the natives to it, and the creature slowly moved on the bottom towards the nets, which he no sooner touched than he quietly turned back and proceeded up the stream. This movement was several times repeated, till, having no rest in the enclosure, he attempted to climb up the bank. On receiving a ball in the body, he uttered a growl like that of an angry dog, and plunging into the water, crossed to the other side, where he was received with a similar salutation, discharged directly into his mouth. Finding himself attacked on every side, he renewed his attempts to ascend the banks, but whatever part of him appeared was bored with bullets, and feeling that he was hunted, he forgot his own formidable means of attack, and sought only safety from the troubles which surrounded him.

A low spot, which separated the river from the lake a little above the nets, was unguarded, and we feared that he would succeed in escaping over it. It was here necessary to stand firmly against him; and in several attempts which he made to cross it, we turned him back with spears, bamboos, or whatever first came to hand. He once seemed determined to force his way, and foaming with rage, rushed with open jaws, and gnashing his teeth, with a sound too ominous to be despised, appeared to have his full energies aroused, when his career was stopped by a large bamboo thrust violently into his mouth, which he ground to pieces, and the fingers of the holder were so paralyzed, that for some minutes he was incapable of resuming his gun. The natives had now become so excited as to forget all prudence, and the women and children of the little hamlet had come down to the shore to share in the general enthusiasm. They crowded to the opening, and were so unmindful of their danger, that it was necessary to drive them back with some violence. Had the monster known his own strength, and dared to have used it, he would have gone over that spot with a force which no human power could have withstood, and would have crushed or carried with him into the lake about the whole population of the place.

It is not strange that personal safety was forgotten in the excitement of the scene. The tremendous brute, galled with wounds and repeated defeat, tore his way through the foaming water, glancing from side to side, in the vain attempt to avoid his foes, then rapidly ploughing up the stream, he grounded on the shallows, and turned back frantic and bewildered at his circumscribed position. At length, maddened with suffering, and desperate from continued persecution, he rushed furiously to the mouth of the stream, burst through two of the nets, and I threw down my gun in despair, for it looked as though his way at last was clear to the wide lake. But the third net stopped him, and his teeth and legs had got entangled in all. This gave us a chance of closer warfare with lances, such as are used against the wild buffalo. We had sent for this weapon at the commencement of the attack, and found it much more effectual than guns. Entering a canoe, we plunged lance after lance into the alligator, as he was struggling under the water, till a wood seemed growing from him, which moved violently above, while his body was concealed below. His endeavours to extricate himself lashed the water into foam, mingled with blood; and there seemed no end to his vitality, or decrease to his resistance, till a lance struck him directly through the middle of the back, which an Indian, with a heavy piece of wood, hammered into him, as he could catch an opportunity. My companion, on the other side, now tried to haul him to the shore, by the nets to which he had fastened himself, but had not sufficient assistance with him. As I had more force with me, we managed, with the aid of the women and children, to drag his head and part of his body on to the little beach, where the river joined the lake, and giving him the "coup de grace," left him to gasp out the remnant of his life on the sand. I regret to say, that the measurement of the length of this animal was imperfect. It was night when the struggle ended, and our examination of him was made by torch-light. I measured the circumference, as did also my companion, and it was over eleven feet immediately behind the fore-legs. It was thirteen feet at belly, which was distended by the immoderate meal made on the horse. As he was only partly out of the water, I stood with a line at his head, giving the other end to an Indian, with directions to take it to the extremity of the tail. The length so measured was twenty-two feet, but at the time I doubted the good faith of my assistant, from the reluctance he manifested to enter the water, and the fears he expressed that the mate of the alligator might be in the vicinity. From the diameter of the animal, and the representations of those who examined him afterwards, we believed the length to have been about thirty feet. As we intended to preserve the entire skeleton, with the skin, we were less particular than we otherwise should have been. On opening him, we found, with other parts of the horse, three legs entire, torn off at the haunch and shoulder, which he had swallowed whole, besides a large quantity of stones, some of them of several pounds weight.

The night, which had become very dark and stormy, prevented us from being minute in our investigation; and leaving directions to preserve the bones and skin, we took the head with us and returned home. This precaution was induced by the anxiety of the natives to secure the teeth; and I afterwards found that they attribute to them miraculous powers in the cure or prevention of diseases.

The head weighed near three hundred pounds; and so well was it covered with flesh and muscle, that we found balls quite flattened, which had been discharged into the mouth and at the back of the head, at only the distance of a few feet, and yet the bones had not a single mark to shew that they had been touched.

At the time of our expedition against the alligator, the periodical visitation of locusts, which occurs about once in seven years, was devastating parts of the island; and, on the following day, the place where I resided was doomed to share in the distress. We were flattering ourselves that the scourge would not come near us, when the dark clouds were seen far over the lake approaching noiselessly, save in the rushing of wings, and soon the sun was hid, and night seemed coming before her time. Mile upon mile in length moved the dark broad column of this insect army; and the cultivator looked and was silent, for the calamity was too overwhelming for words. The sugar cane, the principal crop of that country, gave promise of unusual productiveness when the destroyer alighted. In a moment nothing was seen over the extended surface but a black mass of animated matter, heaving like a sea over the hopes of the planter. And when it arose to renew its flight in search of food for the hungry millions who had had no share in the feast, it left behind desolation and ruin. Not a green thing stood where it had been, and the very earth looked as though no redeeming fertility was left to it. Human exertions availed nothing against this enemy; wherever he came he swept like a consuming fire, and the ground appeared scorched by his presence. Branches of trees were broken by the accumulated weight of countless numbers, and the cattle fled in dismay before the rolling waves of this living ocean. The rewards of government, and the devices of the husbandman, for his own protection, were useless. Myriads of these insects were taken and heaped together, till the air for miles was polluted, without apparent diminution of their numbers.

The typhon was the irresistible agent which at last terminated their ravages, and drove them before it far into the Pacific. This remedy prostrated what the locust had left, but still it was prayed for as a mercy, and received with thanksgiving.

Of the Philippine islands, Luconia is the one best known; but the world of nature there is yet unexplored, and the few men of science who have been permitted to carry their researches into the interior, have either been too easily satisfied with the wonders they encountered at the outset, or have not been spared to give the result of their labours. The one best fitted for the work, who visited that country during my residence in it, was an Italian. He penetrated where the white man had not been seen since the earliest days of the colony, when the followers of Magellan made the circuit of the island with the daring spirit of investigation which distinguished that age of discovery.

He made his way to the wandering Negro tribes which roam through a tract of mountain country near the middle of the island; and who, uninfluenced by the semi-civilization around them, pass an erratic life without fixed habitations, gathering their food from the wild fruit-trees, and offering wide field for conjecture on their origin and insulated position.

The individual I allude to returned from his interesting excursions stored with most valuable information. His indefatigable spirit was undaunted at the great plan he had laid out before him, and he left Manilla with the determination to penetrate to the centre of Borneo—that unknown world, whose savage inhabitants have not been overcome or softened, even by the cupidity of commerce, and whose resources can only be imagined from its magnitude, situation, and the exceeding fruitfulness of its coasts. He had scarcely entered on his new discoveries, when approaching too near a volcano, he slipped into the hot ashes of its burning crater, which in a few days caused his death.

If, in recurring to some of the incidents of my life in Luconia, I have inclined to dwell on what may seem irrelevant to the object of this communication, it is that I am fond of remembering the days I have passed in the solitudes of that lovely land. The dreams of fancy have never pictured scenes of more romantic beauty than are there lavishly spread around; where the principle of life is profusely scattered, and every thing is glowing with animated being—where the bland air makes mere existence enjoyment; and the day, with its mild sky and refreshing sea breeze, gives place to the more serene night, with her clear brilliancy, when the eye looks deep into heaven, and the stars glitter with a radiance unknown in less genial climes—where the land wind rises, and is felt, but not heard, for the stillness of midnight is not broken as its soft breath comes from the untrodden depths of the wilderness, laden with the fragrance of the spice tree and the wild flower.

But in that luxurious region, Nature at times shews herself in the power and sublimity of her convulsions, and awes by the earthquake, the tornado, and the thunder-storm. Her hours of anger are fearful, but are soon forgotten as she resumes her almost permanent tranquillity.*

Researches on Embryology; Third Series: A Contribution to the Physiology of Cells. By MARTIN BARRY, M. D., F. R. SS. L. & E., Member of the Wernerian Natural History Society, Fellow of the Royal College of Physicians in Edinburgh.

In the second series of these researches, of which an account is given in the twenty-seventh volume of this Journal, the author had traced certain changes in the mammiferous ovum consequent on fecundation. The object of his present very interesting communication, which was read before the Royal Society of London on the 7th of May 1840, is to describe their further appearances, obtained by the application of higher magnifying powers; and to make known a remarkable process of development thus discovered. In order to obtain more exact results, his observations were still made on the same animal as before, namely, the rabbit, in the expectation that, if his labours were successful, it would be comparatively easy to trace the changes in other mammals. By pursuing the method of obtaining and preserving ova from the Fallopian tube, which he recommended in his last paper, he has been enabled to find and examine 137 more of these delicate objects; and has thus had ample opportunity of confirming the principal facts therein stated. He has now procured in all 230 ova from the Fallopian tube. But being aware that repeated observations alone do not suffice in researches of this nature, unless extended to the very earliest stages, he again specially directed his attention to the ovum while it is still within the ovary, with a view to discover its state at the moment of fecundation, as well as immediately before and after that event.

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* Silliman's American Journal of Science and Arts, vol. xxxviii. p. 315.

The almost universal supposition, that the Purkinjian or germinal vesicle is the essential portion of the ovum, has been realized in these investigations; but in a manner not anticipated by any of the numerous conjectures which have been published. The germinal vesicle becomes filled with cells, and these again become filled with the foundations of other cells; so that the vesicle is thus rendered almost opaque. The mode in which this change takes place is the following, and it is one which, if confirmed by future observation, must modify the views recently advanced on the mode of origin, the nature, the properties, and the destination of the nucleus in the physiology of cells. It is known that the germinal spot presents, in some instances, a dark point in its centre. The author finds that such a point is invariably present at a certain period; that it enlarges, and is then found to contain a cavity filled with fluid, which is exceedingly pellucid. The outer portion of the spot resolves itself into cells; and the foundations of other cells come into view in its interior, arranged in layers around the central cavity; the outer layers being pushed forth by the continual origin of new cells in the interior. The latter commence as dark globules in the pellucid fluid of the central cavity. Every other nucleus met with in these researches has seemed to be the seat of changes essentially the same. The appearance of the central portion of the nucleus is, from the above process, continually varying; and the author believes that the nature of the nucleolus of Schleiden is to be thus explained. The germinal vesicle, enlarged and flattened, becomes filled with the objects arising from the changes in its spot; and the interior of each of the objects filling it, into which the eye can penetrate, presents a repetition of the process above described. The central portion of the altered spot, with its pellucid cavity, remains at that part of the germinal vesicle which is directed towards the surface of the ovum, and towards the surface of the ovary. At the corresponding part, the thick transparent membrane of the ovum in some instances appears to have become attenuated, in others also cleft. Subsequently, the central portion of the altered spot passes to the centre of the germinal vesicle ; the germinal vesicle, regaining its spherical form, returns to the centre of the ovum, and a fissure in the thick transparent membrane is no longer seen. From these successive changes, it may be inferred that fecundation has taken place ; and this by the introduction of some substance into the germinal vesicle from the exterior of the ovary. It may also be inferred, that the central portion of the altered germinal spot is the point of fecundation. In further proof that such really is the case, there arise at this part two cells, which constitute the foundation of the new being. These two cells enlarge, and imbibe the fluid of those around them, which are at first pushed further out by the two central cells, and subsequently disappear by liquefaction. The contents of the germinal vesicle thus enter into the formation of two cells. The membrane of the germinal vesicle then disappears by liquefaction.

Each of the succeeding twin cells presents a nucleus, which, having first passed to the centre of its cell, resolves itself into cells in the manner above described. By this means, the twin cells, in their turn, become filled with other cells. Only two of these in each twin cell being destined to continue, the others, as well as the membrane of each parent-cell, disappear by liquefaction, when four cells remain. These four produce eight, and so on, until the germ consists of a mulberry-like object, the cells of which do not admit of being counted. Nor does the mode of propagation continue the same with reference to number only. The process inherited from the germinal vesicle by its twin offspring, reappears in the progeny of these. Every cell, whatever its minuteness, if its interior can be discerned, is found filled with the foundations of new cells, into which its nucleus has been resolved. Together with a doubling of the number of the cells, there occurs also a diminution of their size. The cells are at first elliptical, and become globular.

The above mode of augmentation, namely, the origin of cells in cells, appears by no means to be limited to the period in question. Thus it is very common to meet with several varieties of epithelium-cells in the oviduct, including those which carry cilia, filled with cells; but the whole embryo at a subsequent period is composed of cells filled with the foundations of other cells.

In the second series of these researches, it was shewn that the mulberry-like object above mentioned is found to contain a cell larger than the rest, elliptical in form, and having in its centre a thick-walled hollow sphere, which is the nucleus of this cell. It was further shewn that this nucleus is the rudimental embryo. From what has been just stated, it appears, that the same process by which a nucleus in one instance transforms itself into the embryo, is in operation in another instance, where the product does not extend beyond the interior of a minute and transitory cell. Making allowance, indeed, for a difference in form and size, the description given of the one might be applied to the other. It was shewn in the second series, that in the production of the embryo out of a nucleus, layer after layer of cells come into view in the interior, while layers previously formed are pushed further out; each of the layers being so distinctly circumscribed as to appear almost membranous at its surface. The same membranous appearance presents itself at the surface of the several layers of a nucleus in many situations. Further, in the formation of the embryo, a pellucid centre is the point around which new layers of cells continually come into view; a centre corresponding to that giving origin to similar appearances in every nucleus described in the present memoir. It was shewn that in the embryo this mysterious centre is present until it has assumed the form of the cavity, including the sinus rhomboidalis, in the central portion of the nervous system.

The process above described, as giving origin to the new being in the mammiferous ovum, is no doubt universal. The author thinks there is evidence of its occurrence in the ova of batrachian reptiles, some osseous fishes, and certain of the mollusca; though the explanation given of these has been of a very different character. It has hitherto been usual to regard the round white spot, or cicatricula, on the yolk of the bird's laid egg, as an altered state of the discus vitellinus in the unfecundated ovarian ovum. So far from thinking that such is the case, the author believes the whole substance of the cicatricula in the laid egg to have its origin within the germinal vesicle, in the same manner as in the ovum of mammalia.

There is no fixed relation between the degree of development of ova, and their size, locality, or age. The variation with regard to size is referable chiefly to a difference in the quantity of fluid imbibed in different instances by the incipient chorion. Vesicles filled with transparent fluid are frequently met with in the Fallopian tube, very much resembling the thick transparent membrane of the ovarian ovum. These vesicles are probably unimpregnated ova, in the course of being absorbed. The so-called "yelk" in the more or less mature ovarian ovum, consists of nuclei in the transition state, and exhibiting the compound structure above described. The mass of these becomes circumscribed by a proper membrane. They and their membrane subsequently disappear by liquefaction, and are succeeded by a new set, arising in the interior, and likewise becoming circumscribed by a proper membrane, and so on. This explains why some observers have never seen a membrane in this situation. After the fecundation of the ovum, the cells of the tunica granulosa, that is, part of the so-called "disc," are found to have become club-shaped, greatly elongated, filled in some instances with cells, and connected with the thick transparent membrane by their pointed extremities alone.

That the thin membrane described by the author in his second series as rising from the thick transparent membrane in the Fallopian tube, and imbibing fluid, is really the incipient chorion, was then shewn by tracing it from stage to stage, up to the period when villi form upon it. There remained, however, two questions undecided; viz., whether the chorion is formed of cells, and if so, whether the cells are those of the so-called "disc," brought by the ovum from the ovary. The author now states that the chorion is formed of cells, which gradually collect around the thick transparent membrane, and coalesce; and that the cells in question are *not* those of the "disc," brought with the ovum from the ovary. The cells which give origin to the chorion are intended to be more particularly described in a future paper.

The existing view, namely, that a nucleus, when it leaves the membrane of its cell, simply disappears by liquefaction, is inapplicable to any nucleus observed in the course of these investigations. The nucleus resolves itself into incipient cells in the manner above described. In tracing this process, it appears that the nucleus, and especially its central pellucid cavity, is the seat of changes which were not to have been expected from the recently advanced doctrine, that the disappearing nucleus has performed its entire office by giving origin at its surface to the membrane of a single cell. It is the mysterious centre of a nucleus which is the point of fecundation; and the place of origin of two cells constituting the foundation of the new being. The germinal vesicle, as already stated, is the parent cell, which, having given origin to two cells, disappears, each of its successors giving origin to other two, and so on. Perpetuation, however, at this period, consists, not merely in the origin of cells in cells, but in the origin of cells in the pellucid central part of what had been the nucleus of cells.

The author shews that neither the germinal vesicle, nor the pellucid object in the epithelium, is a cytoblast. He suggests, that the cells into which, according to his observations, the nucleus becomes resolved, may enter into the formation of secondary deposits, for instance, spiral fibres; and that they may contribute to the thickening which takes place, in some instances, in the cell-membrane.

The germ of certain plants passes through states so much resembling those occurring in the germ of mammiferous animals, that it is not easy to consider them as resulting either from a different fundamental form, or from a process of development which, even in its details, is not the same as what has been above described; the fundamental form in question in Mammalia, and, therefore, it may be presumed, of Man himself—being that which is permanent in the simplest plants—the single isolated cell.

On the Frequency of Thunder-Storms in the Polar Regions.

By M. Von Baer.

M. Arago, in his elaborate work on thunder, published in the year 1838, has stated it as an established fact, "*that, in the wide ocean, and in islands, it never thunders beyond the* 75° *of N. latitude; and that at the* 70° *the phenomenon is very seldom heard—scarcely once a-year.*" M. de Baer, who was travelling in the extreme north of Europe in 1837, having had occasion to make observations which exhibited the limit of storms, if such limit there is, much nearer to the pole, has communicated to the Imperial Academy of Petersburg in a note, and under form of a letter, dated the 7th of May, to M. Jacobi, the remarks he himself has made on this subject, and those he has collected from other authors. Of these we shall supply a summary in as few words as possible.

M. de Baer commences by directing attention to the circumstance, that M. Arago has based his general conclusion upon an insufficient number of documents. "In truth," he remarks, "M. Arago has only consulted the English voyages undertaken in our own times to the northern parts of America and Spitzbergen, and the observations made in Iceland by M. Thorstensen for a period of two years only. At the same time, many of these voyagers were for a long time on the wide ocean, or in islands of no great extent, and no one knows better than M. Arago that the farther you are removed from continents, the more rarely do you encounter thunder-storms. But, in addition to this, more extended observations prove, that in the very same spots where these passing voyagers have not heard thunder, it has from time to time been heard by others, so that this proposition may be stated, '*That no northern latitude has been attained by man in which thunder is not known to occur.*' It even thunders at Spitzbergen, though certainly very rarely. In Nova Zembla, I have witnessed a thunder storm beyond the 73°, and the narratives of the hunters of the walrus contain many accounts of it."

We shall commence, however, with Iceland. M. Arago's notice of this island is as follows:—"Iceland is often adduced as a country in which thunder never occurs. The word *never* here used, however, should be altered, for in an interval of about two years we find, in the meteorological observations of M. Thorstensen, there was one day, the 30th of November, in which thunder was heard." Upon this sentiment M. Baer remarks, "That if there be those who adduce Iceland as a country where no thunder occurs, it is evident that they have not consulted the numerous works which have been written regarding it. Thunder-storms are in reality frequent there, though not so common as in most other parts of Europe. It is not to be forgotten, say Olafsen and Povelsen, as a memorable fact, connected with the northern part of Iceland, that thunder-storms prevailed there, and terrible lightnings, *during the whole of the summer* of the year 1718, and that, on the 11th of June, a man was killed by a thunderbolt. The same observers, who dwelt long in Iceland, assert, that lightnings are frequent in the northern parts, and that thunder is heard there from time to time. In the western peninsula storms are rare, and thunder is only heard at distant intervals of time. Thunder is more frequent on the southern portion of the island; there it has twice destroyed the cathedral of Skalholt, and in 1634, it removed the roofs from a number of houses. Anderson* tells us that it thunders in those countries usually only in winter; and Olafsen and Povelsen† repeat the assertion.

"In Greenland, thunder is more uncommon, according to the statements of Egède and Crantz.‡ The former of these missionaries resided in the country for fifteen years; and the latter remarks, that you sometimes observe lightnings, but thunder is but seldom heard.

"Upon the continents in the same latitude as Iceland, thunder is much more frequent than in that island. In America, on the coasts of Hudson's Bay, Ellis and James Hudson have witnessed thunder-storms.§ On the thunder-storms of Labrador, the observations of Latrobe may be consulted.* M. Arago founds his conclusion upon the circumstance that the observations of Captain Franklin, made at Fort Franklin, in latitude 65° 12′ N. (and not in 67½°, as he has indicated by mistake), and which state the occurrence of thunder only once (29th May) during a whole year; but he seems to have overlooked the fact, that the month of June is omitted in the tables, the month in which, of all others, such storms are apt to occur in the northern regions of the Continent.† In Europe, in the same latitudes as Iceland, thunder-storms are still more frequent. Julin heard thunder eighty-eight times during twelve years, at Uleaborg (Lat. 65° N.), in other words, 7.3 times a-year.‡ At Archangel (Lat. 64° 34′ N.), according to the observations of M. Kupffer, there were 121 thunder-storms in eighteen years, or in the ratio of 6.5 a-year. At Beresow (Lat. 64°) there were, in 1832, six storms. At Jakutsk (Lat. 62° N.) it thundered six times in the year 1838. At Nertschinsk it occurred eighteen times in six years, or at the rate of 3 times a-year. These observations seem to prove, that the frequency of thunder-storms is regulated by the isothermal, or still more by the isotheral lines, rather than by the degrees of latitude.

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* Anderson; Nachrichten von Island, Grœnland und der Strasse Davis, 1747, s. 123.

† Voyage en Islande fait par ordre, &c. S. M. D. t. I. p. 13.

‡ Egede, Beschreibung und Naturgeschichte von Grönland, s. 79. Crantz Historie von Grœnland, b. i. s. 62.

§ Scoresby's Account of the Arctic Regions, vol. i. p. 415.

"To these facts we now add a few other observations which were made further to the north, and which prove that, in Europe, thunder-storms are not so rare as they appear to be in America, according to the observations of Captain Franklin. We first extract some of the notes of M. Schrenk, a travelling botanist, who, in the year 1837, traversed the territories of the Samoyedes:—'On the 3d of June, upon the banks of the river Rotschuga, Lat. 65½° N. experienced a thunder-storm: on the 8th of June, on the banks of the Sylma, same latitude, thunder occurred along with rain, but without lightning;‖ on the 17th July there was another thunder-storm, in the deserts destitute of trees, which are called *Tundras* in Russia, in the latitude of 68° and a trifle more; and, lastly, on the 21st July, in the latitude 69°, the same traveller witnessed much lightning, but without thunder. During a journey of a week's continuance, which was made the same year in Russian Lapland, along with M. Lehman, there was on the 23d of June, near the mouth of the river Ponoi, at an inhabited port which is called Tri Ostrowa, Lat. 67°, a thunder-storm which continued for three hours, and which appeared to extend as far as Lat. 68° N. M. Reinecke, Captain of the Russian Marine, who, during his examination of the coasts of the White Sea and of Russian Lapland, dwelt at Kola, and upon the north coasts of that part of Lapland, from the middle of March to the end of summer, heard in this country, between 69° and 70°, eight storms in 1826; and at Utsioki, Lat. 70°, Wegelius heard thunder thrice in the year 1758.

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* Phil. Transact. vol. lxix. and vol. lxxi.

† Franklin's Narrative of a Second Expedition to the Shores of the Polar Sea, in the years 1825, 1826, 1827. Append. lxix and lxx.

‡ Der Köngl. Schwed. Acad. Neue Abhandlungen. Bd. x. (Jahrg. 1789) S. 109.

‖ Georgi's Bemerkungen einer Reise im Russ: Reiche, Bd. i. s. 427 to 435.

"If it be true that there are a greater number of storms in the interior of the Arctic countries than upon their shores, they, nevertheless, are not altogether wanting even in the midst of the Polar ice. Admiral Wrangell, in one of the perilous journeys which he made upon the ice of the Polar Sea, to the north-east of Siberia, observed a thunder-storm upon the ice, even when out of sight of land.

"M. Ziwolka and I witnessed a storm in the centre of Nova Zembla, at the western embouchure of Matotschkin-Schar Strait, in Lat. 73° 10′, on the 26th of July 1837. Rakhmanine heard thunder three times in the southern part of Nova Zembla, between the latitudes 71° and 73¼°, in the course of the voyages he made during the last century, and which occupied two summers and twenty-six winters.

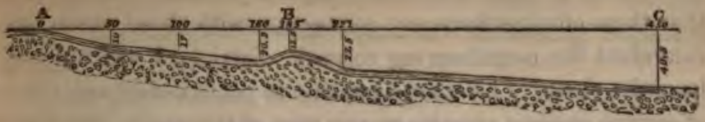
"Finally, it sometimes thunders at a higher latitude than 75°, and even as far north as Spitzbergen. This we learn from the recital of four shipwrecked Russians, who found an asylum on the eastern isle of Spitzbergen, on which three of them lived for six years and three months. They heard thunder once, but once only, during this long period."

Notice of Elevated Sea-Beaches. By ALAN STEVENSON, LL.B., F.R.S.E., Civil Engineer. Communicated in a Letter to Professor Jameson.

SKERRYVORE LIGHTHOUSE WORKS,
20 Miles Seaward of Iona,
April 28. 1840.

MY DEAR SIR,

As you invited me, when I had the pleasure of seeing you before I left Edinburgh for my summer expedition, to communicate (no matter how imperfectly) any observations which might occur to me, or facts which might come under my notice in my exile, I beg leave to trouble you with the accompanying section of two ancient beaches in the Island of Mull, which are situated not far from the quarries, the property of his Grace the Duke of Argyle, that supply the materials for Skerryvore Lighthouse. My attention was directed to this spot by an intelligent person, Mr Charles Barclay, who is foreman of our quarries, and who recognised in the spot to which the sketch refers, a resemblance to some of the principal features described by Captain Basil Hall, as having been observed by him on the coast of Chili. This beach is in a district of the parish of Bunessan, called the *Ross of Mull*, about half a mile NW. from the loch where we opened our quarries. The district of the *Ross* is entirely composed of granite, which closely resembles, both in its structure and the mode of its arrangement, the celebrated granite of Peterhead; and as it is in some places tinged of a deep red, and but thinly and partially covered with moss, it gives the whole coast a reddish appearance, from which I believe it has acquired its name. The length of the face of the lower of these two beaches is about 700 feet, and its distance from the highest point reached by the tide is about 250 feet, while the level of its top is 25 feet above the highest level of any tide that I could trace. What I suppose to be a second beach, is less distinctly defined than the lower one, and is situated fully 40 feet above the level of high water. It is more extensive in its *face-wall* (if I may so speak); but I did not take any measurement of its length. The heights in the accompanying section were determined by actual measurement with a spirit-level.



A is the top of the higher Ancient Beach, which is 40.5 feet above the present high-water of spring-tides at C.

B is the top of the lower Ancient Beach, which is 25.3 feet above the present high-water of spring-tides at C.

The dimensions in the section are given in feet and decimals.

I have but little time to speculate on the subject of these beaches, and I shall therefore only trouble you with a few observations which occurred to me on the spot. The upper beach is terminated at both ends by the hill country of the *Ross*, and is also, in its middle, broken by a round hillock which on one side appears to be rising out of the level plain which forms the top of the beach. Upon digging, I found the same species of boulders as those which lie on the present shore, and they were only covered with a thin but tough coat of mossy grass, which the spade penetrated with difficulty.

The lower beach has much more marked features than the upper one; and its appearance is such as can hardly fail to strike the most careless observer. It has somewhat the look of an artificial mound, the ground being lower at the back of it than at its face or crest. Two projecting tongues, in which the adjoining lines of the beach unite, are very remarkable, and are obviously produced by the action of the waves from the sides of two small bays opposite each other, and an inlet between them, at each of which the face of the beach is concave. This beach is also composed of boulders, which, however, appear more rounded than those of the other beach, or indeed than those which compose the present shore. This may arise from the stones having been longer exposed to the action of the sea before being raised to their present level. The contour of both these beaches, but more especially that of the lower one, is characterized by the slightly concave form which distinguishes sea-beaches composed of gravel and sand; and I have not the least doubt that these ridges are of marine origin, while their position, in an almost land-locked bay, renders it highly improbable that the accumulation of gravel could have been produced by any temporary storm. As to whether these changes have any connection with those which have convulsed the neighbouring volcanic districts, I will not even hazard a guess. I have not been able as yet to detect any traces of marine organic remains on any part of this coast, although I looked for them in connection with the levels of the beaches, nor could I find any traces of a vertical dislocation of the strata in the neighbourhood. This, however, need not discourage others, as my glances were very hasty. I have engaged Mr Barclay, who enters zealously into the inquiry, to exaamine the neighbouring rocks, with the view of detecting any appearances which may be interesting in connection with the *beaches*, or may in any way tend to illustrate their history; and I shall not fail to apprize you of any thing he may pick up in his frequent perambulations of the coast; nor shall I neglect, when my affairs again call me to Mull, to prosecute the subject as far as I can. I am, my dear Sir, very faithfully yours,

ALAN STEVENSON.

Professor Jameson, &c. &c. &c. Edinburgh.

On the Difference of Level between the Dead Sea and the Mediterranean.

It is a remarkable coincidence, that, at the very time when the last Russian Expedition was occupied in determining the problem of the relative level of the Caspian Sea, a less extensive, but equally interesting example of a great depression was discovered in Palestine. Professor Schubert of Munich, two Englishmen, Messrs Moore and Beek, and M. J. de Bertou, a Frenchman, almost simultaneously, and quite independently of one another, have made the discovery, that the Dead Sea, and likewise the entire lower valley of the Jordan, are situated considerably under the level of the Mediterranean Sea. The newspapers have already made occasional mention of this discovery, but the scientific journals have hitherto been silent on the subject, owing to the absence of authentic information. We are now, however, in possession of the latter, and it appears to be desirable, to present a short analysis of the present state of our knowledge upon this interesting subject of physical geography.

In the third volume of his Travels in the East in 1836 and 1837 (*Reise in das Morgenland in den Jahren* 1836 *and* 1837. *Erlangen* 1839), Professor Schubert, among other barometrical results, gives the following :—

	Parisian Feet above the Sea.	Parisian Feet under the Sea.
Edge of the mountains of the upper valley of the Jordan, in a limited sense, . .	858	
Jacob's Bridge on the Jordan, . .	350	
Lake of Tiberias or Genezareth, . .		535
Plain of the Jordan near Jericho, . .		528
Northern corner of the Dead Sea, . .		600

During the last observation, the mercury ascended beyond the scale of the barometer, which was not suited for so great a rise, and hence its height could only be calculated by the eye. The observations moreover, are only incidental, in Schubert's work,* and the data afforded by observation are not given, although a detailed account is promised in the Munich Transactions.

The observations of M. Bertou are shortly given in *the Bulletin de la Société de Géographie*, Jan. 1839, vol. x. p. 274, &c. The following series of results is extracted from them.

Date and Locality.		Barometer.		Temperature. Cent.
		Parisian Inches.	Millimètres.	
March 3.	Beirut, ...	28	757.96	21°
6.	Sidon, ...	28	757.96	...
6.	Acre (Latin Monastery).	27.03	737.66	16
12.	Jerusalem, . . .	25.09	697.05	15¾
12.	Jericho, ...	28.11	782.78	21¼
13.	Jericho, ...	29.00	785.03	13¾
13.	Northern extremity of the Dead Sea,	29.06	798.56	21¼
13.	Jericho, . . .	29.00	785.08	27½
14.	Jerusalem, . . .	25.09	697.05	16

The numbers here are inches and lines of the old French measurement, as is evident from the column of millimetres. M.

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* The following is the account of the level of the Dead Sea given by Dr Gotthilf Heinrich von Schubert in his amusing narrative :—" We were not a little astonished at Jericho, and still more at the Dead Sea, to see the mercury in our barometer (which, as I shall afterwards mention more particu-

Bertou has thus read off no smaller parts than whole lines; and neither the temperature of the barometer, nor that of the air, seems to have been determined. His observations, therefore, have no claim to great accuracy.* Notwithstanding this, and perhaps other deficiencies in the measurements, the great height of 29½ Parisian inches, observed at the Dead Sea, can only be explained by its low position.

In the *Comptes Rendus*, vol. vii. p. 798, M. Callier has calculated the above-mentioned height of the barometer (which, however, he gives as only 797^{mm}.5, instead of 798^{mm}.56), and thence, assuming 760^{mm}.0 as the mean height of the barometer at the level of the sea, he has deduced 406 metres = 1249.8 Parisian feet (about 1330 English feet) for the depression of the Dead Sea; which is more than double the amount assigned by Professor Schubert.

Shortly after his first excursion to the Dead Sea, a little air unfortunately entered M. Bertou's barometer, and he therefore performed his subsequent measurements, during a journey from Hebron to Akaba and back again, by means of the boiling point of water, as determined by a thermometer of Lede-

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larly, was not adapted to such observations) ascend beyond the scale. We were obliged to calculate the height by the eye, and although we reduced the calculation as much as possible, owing to the extremely unexpected nature of the result, yet the level of the Dead Sea hence deduced, was at least 598¼, or, in round numbers, 600 Parisian feet: that is nearly 640 English feet under that of the Mediterranean. We endeavoured to explain away this conclusion in every possible way. First of all, we supposed an accidental unusually high state of the barometer on the very day of our observation; but the previous day's storm ought rather to have produced a fall than a rise of the mercury. We next imagined that there must be something wrong with our instrument, exposed as it had been to so many mischances; but, on our return to Jerusalem, it exhibited the same mean height as before our journey to Jericho. Notwithstanding all this, however, I could not have ventured to make public so extraordinary a measurement after my return home, although the measurement of the height of the Lake of Tiberias corresponded with it, had it not been, that some of my friends published a notice of it in the *Allgemeine Zeitung*. And scarcely had this taken place, when immediately public confirmations made their appearance, of the abnormal depression of the Dead or Salt Sea, first of all from Mr Beek, and afterwards from other observers; confirmations which, nevertheless, make our determination appear the most moderate." (Vol. iii. p. 86).—EDITOR.

* M. Bertou himself is surprised at the great difference of the results for *Sidon and Acre*, two places lying on the sea-coast.

bours, which was evidently not accurate enough for such a purpose. Of these measurements, the following have the greatest interest :—

		Boiling point.	Temp. of the air.
April 3.	Southern extremity of the Dead Sea,	100°.6	32°.6 Cent.
6.	Water-shed, el Sateh, ...	98.0	25
8.	Akaba, on the Red Sea, . . .	99.9	29
May 1.	Jerusalem.	96.0	20

The boiling point at 100°.6 C. corresponds to a height of the barometer of 776$^{\text{mm}}$.45 (at 0°), and gives for the Dead Sea a depression = 166 metres = 510 Parisian feet (upwards of 540 English feet), a result which, if either of the previous ones be at all correct, must be much under the truth.

But the result obtained by thermo-barometric means, by Messrs Moore and Beek, differs still more from the other determinations. According to M. Callier (*Comptes Rendus*, vol. vii. p. 798) these observers found the boiling point on the banks of the Dead Sea = 216°.5 F. = 102°.5 Cent. This temperature would indicate, according to him, an atmospheric pressure of 815$^{\text{mm}}$.6, and assuming a mean height of the barometer of 760$^{\text{mm}}$.0 at the level of the sea, would give for the depression of the Dead Sea 608 metres = 1872 Parisian feet (nearly 2000 English feet),* an estimate three times as great as that by Professor Schubert.

From these data it is clear, that although there can be no doubt of the low position of the Dead Sea, yet that the amount of the depression is as yet by no means ascertained. Travellers have lately started from England for Palestine provided with good barometers; but it is evident that no great certainty can be obtained on the subject, unless observations are made for at least some months, and unless corresponding observations are made at some neighbouring point on the coast of the Mediterranean, which have been awanting in all the measurements hitherto made.

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* The amount of depression deduced by Messrs Moore and Beek themselves, from their own observations, is very different from the result of their experiments given by M. Callier, as appears from the following extract from Mr W. R. Hamilton's address delivered to the Royal Geographical Society of London, on the 27th May 1839. "The exact level of the surface of the Dead Sea, is a point of increasing interest not yet satisfactorily cleared up. Mr Moore, by thermometric observations, has estimated it at about 500 feet below the level of the Mediterranean; Professor Schubert, by barometric observations, at 600 feet; whilst Mr Russegger, an Austrian naturalist, has, also from barometric observations, recently stated it to be at a depression of no less than 1400 feet below the Mediterranean; but we trust that this point will not long remain a stumbling-block for geographers, as I am happy to acquaint you that more than a month since, your Secretary, not unmindful of the interest attached to what appears to be one of the most remarkable features in the physical geography of the globe, placed an excellent barometer, made by Newman, and compared with the Royal Society's standard, in the hands of two young Englishmen about to visit Palestine, with a special request that they would endeavour to settle the point in question."—EDITOR.

The fact of the depression of the Dead Sea will, at the same time, decide another question, which has of late years been a subject of discussion among geographers. It is well known that it was discovered in 1805 by Seetzen, and afterwards confirmed by Burckhardt, Bankes, and many other travellers, that, from the southern extremity of the Dead Sea, a continuous longitudinal valley, like a trough, runs down to the Gulf of Akaba, the eastern branch of the bifurcation of the Red Sea. This very distinct valley, which has quite the appearance as if it were a continuation of the Gulf, has for some time given rise to the opinion, that the Dead Sea formerly stood in connection with the Red Sea, and hence, that the Jordan formerly flowed into the latter.* The low position in which it is now proved that the Dead Sea is placed, shews, however, that, at least in the present condition of the surface of the earth, it must always have been an inclosed basin. This opinion receives a further confirmation from the nature of the longitudinal valley just spoken of. M. Bertou, who was the first to travel through it in its whole extent, found that it by no means presents a continuous flat surface, but that it consists of three distinctly separated portions, *Wadi el Ghor*, *Wadi el Araba*, and *Wadi el Akaba*; and that, nearly in the middle of its length, it contains a water-shed called *El Sateh* (the roof), whence the streams flow on the one side to the Red Sea, and on the other to the Dead Sea. The northern portion *Wadi el Ghor*, which is, upon the whole, very fruitful, is, as it were, a continuation of the Dead Sea, and salt streams descend from the mountains to its margin, which evidently give rise to the great saltness of this sea.

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* *Vide* Von Hoff's *Geschichte der natürlichen veränderungen der Erdoberfläche*, vol. ii. p. 118; and other authorities.

The middle valley *Wadi el Araba*, has, in its northern portion, a long narrow furrow, which, at the first glance, might be taken for a continuation of the valley of the Jordan, in which, however, in the winter time, the waters flow from the south to the north, towards the Dead Sea. A fuller elucidation of the reasons for the original separation of the basin of the Dead Sea from that of the Red Sea, is given by M. Letronne in the *Nouvelles Annales des Voyages* for 1839.

Finally, it ought further to be mentioned here, that the Dead Sea is distinguished by its great depth; for, in the 7th volume, p. 456, of the Journal of the Geographical Society of London, it is stated, that the soundings of Messrs Moore and Beek shewed a depth of upwards of 300 fathoms.—(Poggendorff's *Annalen der Physik und Chemie*, 1840.)

On account of the great interest excited by the fact of the depression of the Dead Sea, it may not be out of place to introduce here Russegger's account of his observations.—" I suspended, with some degree of eagerness, a barometer on the turret of an old castle in the small town of Richa (Jericho). The mercury, at half-past five o'clock in the evening, and at a temperature of the air of 18° R. (72°.5 F.), stood at 786.1 millimetres (that is 29″ 0‴.42 Parisian measurement), and at eight o'clock in the evening, it stood at 786.8 millimetres (or 29″ 0‴.77) at an atmospheric temperature of 14° R. (63°.5 F.). During the morning, I rode to the Jordan; and at the usual bathing-place of the pilgrims, the barometer ascended to 801.8 millimetres (or 29″ 7‴.42) at an atmospheric temperature at mid-day of 21°.8 R. (81.05° F.) I then proceeded to the coast of the Dead Sea, and on suspending the barometer, I could no longer make an observation, for the mercury stood close to the top of the tube, which was too short for the great depth at which I then was below the level of the sea. Taking into consideration the temperatures of the mercury at each observation, and calculating the average of simultaneous observations at Jerusalem, the following are the results of the heights or depths: The town of Richa in the valley of the Jordan 774 Paris feet (about 825 English feet); bathing-place of the pilgrims at the Jordan 1269 Paris feet (about 1350 English feet); and the Dead Sea *about 50 feet* lower than the last, which is about two English miles distant from it, or 1319 Parisian feet (upwards of 1400 English feet) below the level of the Mediterranean Sea. It ought not to be left without notice, that the results of Schubert, or rather of Erdl, rest only on estimates, and not on observations. Thus, in a manuscript, now lying before me, it is said: '1837, April 12. Jericho, during a violent storm in the evening the mercury stood so high as to be covered by the wooden mounting. As far as could be estimated by inclining it, it must have reached at least to 347‴=28″ 11‴; thermometer 25⅛° R (88°.53 F.). April 13. at mid-day in fine weather, the same circumstance took place at the Dead Sea, that the glass tube was visible for too short a distance, and that the scale could not be observed to a sufficient height. According to the estimate of its height formed by inclining it, the mercury must have stood at 348‴=29″ 0‴; thermometer = 23⅝° R. (85°.15 F.).' From these estimated and not observed heights of the barometer, Steinheil has calculated the depression of Jericho at 527.7, and that of the surface of the Dead Sea at 598.5 Parisian feet under the level of the Mediterranean. As Russegger's barometer still admitted of the actual reading of the height of the mercury, we might certainly be inclined to give his results the preference, or at all events the number assigned by him to Jericho. It seems, however, doubtful, and not reconcilable with appearances, that the Dead Sea should lie 545 feet lower than Jericho. What Russegger termed a castle, at which he made his observations, is a tower of about 30 feet in height, and if we estimate the difference of height between the surface of the earth at Jericho, and that of the Dead Sea, at 80 or 90 (180 or 190?) feet, we obtain for the surface of the latter, at the most, a depression of 990 Parisian feet (upwards of 1050 English feet) under the level of the Mediterranean, which corresponds well with the depression of the Sea of Galilee, deduced from the observations of Erdl. On the 23d April 1837, at 5 o'clock P.M., at the Lake of Genezareth, the height of the mercury was actually observed to be 28″ 10‴.5, at a temperature of 15⅝ R. (67°.15 F.). Hence Steinheil finds the level of the lake to be 535.3 Parisian feet under the level of the Mediterranean; therefore, compared with the above amount (990 feet), we have 455 feet for the height of the lake above the level of the Dead Sea, which gives a considerable but not extravagant amount of descent for the Jordan. Schubert's expedition arrived in Jerusalem, on the 28th March, and quitted the Holy City on the 15th April. During this period, Dr Erdl made 31 barometrical and thermometrical observations, on the first floor of the Latin Monastery of St Salvator. These indicated a variation of only 2″.9; for the greatest height was 310‴.5, and the smallest was 307‴.6. The height of Jerusalem, as deduced from these observations, and according to Steinheil's calculation, is 2472.9 Parisian feet (2634 English feet) above the level of the Mediterranean Sea, which agrees perfectly with Russegger's determination, who assigns a height of 2479 Parisian feet to St Salvator. The highest summit of the Mount of Olives is, according to Erdl, only 83 feet (88 English) higher than the Latin Monastery; but the bed of the Kedron, at the tomb of Jehosaphat is 333 feet (355 English) lower than St Salvator. Damascus lies nearly 2200 feet (2344 English), the pass of Lebanon at Dschebel Makmel 7154 feet (7624 English), and the grove of the cedars of Lebanon, 5878 feet (6264 English) above the Sea.—*(From Berghaus's Almanach, für* 1840.

Report on the Geology of Newfoundland. By J. B. Jukes, Esq. B.A. & F.G.S.*

In the present state of geological science, an observer commencing an investigation of a country at a distance from those which have been already described, is very much in the condition of one who begins the science afresh. The nomenclature and classifications with which he has been familiar, have to be discarded, or at all events held as of uncertain application to the things he has now to examine; and instead of tracing and mapping down a series of rocks the order of which is known, and in which the identification of one affords a ready clue to the interpretation of the rest, the geological surveyor has to labour at long, uninteresting and perplexing details, in order to acquire the preliminary knowledge with which to begin his work. If to these considerations be added that of the difficulties arising from an uncleared country and a dangerous coast, increased during the latter part of the season by unfavourable weather, I hope the small part of the survey which has been completed during the past summer, compared with what I had expected to accomplish, will be sufficiently accounted for.

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* The copy of the report sent to us was accompanied by the following letter:—"*Secretary's Office, Newfoundland, 17th February* 1840.—Sir, I am directed by the Governor, to transmit to you a copy of a report on the Geology of this Island, made by Mr J. B. Jukes, who is prosecuting a geological survey under a provision made for that purpose by the Legislature of the Colony. I have the honour to be, Sir, your obedient humble servant,

(Signed) James Crondy.

"The Professor of Geology in the University of Edinburgh."

The best form into which the materials collected can be thrown, will, I think, be, first of all, a general account of the different formations met with in the course of the survey, and then a sketch of the portions of the country occupied by each, their local varieties, and then their relations one with the other. As I have not yet been able to connect the eastern and western sides of the Island, I will describe them each separately, as far at least as regards the stratified rocks. And inasmuch as any names which can be given to the several formations must be for the present provisional, and I wish by all means to steer clear of that fruitful source of error, hasty generalization, I shall apply to the different formations, names derived either from those places near which they are best exhibited, or from some obvious and general character.*

Stratified Rocks of the Eastern part of Newfoundland.

(In the descending order.)

1. *The Bell Isle Shale and Gritstone Formation.*—This formation is the newest or highest in the series of stratified rocks on the eastern side of the Island. It consists of a great mass of dark brown and black shale, interstratified with beds of a fine-grained gritstone. The shale is of various degrees of hardness, sometimes crumbling beneath the finger and in very thin laminæ, at others in thicker plates, requiring a sharp blow

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* Our readers will find some interesting geognostical information regarding Newfoundland, in vol. x. p. 156 of the Edinburgh Philosophical Journal, by William Cormack, Esq.; and in vol. iv. p. 151 of Memoirs of Wernerian Natural History Society, by Mr John Baird.—Edit.

to break them. It is frequently micaceous, and some portions of it put on precisely the appearance of some mica-slate, having a curved or wrinkled lamination, and being entirely composed of scales of white silvery mica. Some slabs of this shale are covered with singular markings in relief, at first sight resembling the leaves and branches of small plants or sea-weeds; they are, however, I believe, concretionary, and not organic. The softer parts of the shale frequently decompose *in situ* into a dark brown earth, which lodges in the crevices and on the ledges of the cliffs, and has precisely the appearance of fine vegetable mould. The beds of gritstone which occur at various intervals in this mass of shale, are universally fine-grained, grey internally, but weathering brown outside, generally thin-bedded, being rarely more than two feet thick, and are divided by joints into sharp angular blocks. These joints are almost invariably at right angles to each other, and when also perpendicular to the beds, the blocks are of course rectangular, and form good building stone. If not thus naturally square, however, the stone will not readily admit of being made so artificially, as it is of a brittle splintery character. In the upper part of this formation, the shale is much more abundant than the gritstone, which latter frequently occurs in single beds, with regular intervals of shale between each; in the lower portion the beds of gritstone are more grouped together, forming a thickness sometimes of 20 or 30 feet, and the shale bears a less proportion to the stone than in the upper part. The thickness of the whole formation must be considerable: but owing to the want of a continuous section, and other difficulties, it must be left to conjecture. It cannot, however, be so little as 600 feet. The Bell Isle shale and gritstone is in some places seen to graduate or pass down by regular degrees into the next inferior, or that which I shall term the variegated slate-formation. One formation is said to graduate downwards or upwards into another, when, at their junction, the beds of each alternate the one with another, and no positive line of separation can be drawn between the two.

2. The *Variegated Slate-Formation* consists of a mass of rocks, the most remarkable and abundant of which are some bright red and greenish-grey slates. The upper part of this forma-

tion is almost invariably of a very fine grain, but here and there contains coarser beds, or even patches of small conglomerate. The fine-grained beds are generally traversed by a slaty cleavage, but, from their brittle character, seldom split into large slates, and are never sufficiently durable to be used for economical purposes. Some of the beds are slightly calcareous. The bright red colour generally characterizes certain beds, each bed, or group of beds, being only of one hue; sometimes, however, a sudden change takes place, the red colour ending in one or more broad streaks, and the remainder of the mass being greenish-grey. The colours are likewise in variable proportions in different localities; the predominating hue being red in one place, green in another, and becoming in some places brown, cream-coloured, or yellowish. The slaty cleavage is most frequently developed in the upper part of the formation; the lower beds, though retaining something of their characteristic colouring, are rather coarser, more siliceous, and become compact slate-rock or gritstone. The total thickness of this formation must certainly exceed a thousand feet.

3. *The Trinity Bay Sandstone-formation.*—This is the rock which usually occurs next below the variegated slates; I cannot, however, as yet state whether the two pass into each other or not. The Trinity Bay sandstone-formation is composed of materials of which the following section is an example:—

	Feet.
1. Dull red sandstone or gritstone, containing a few pebbles, in enormously thick beds, some being so much as 30 or 40 feet,...	400
2. Alternating beds of coarse and fine grained rock, the finer beds exhibiting an imperfect slaty cleavage, and the beds generally very thin, sometimes not more than three inches,..................	400
3. Dark red sandstone,...... Light purple ditto, ... Dull red sandstone and conglomerate,...... Gritstone with a dull red and white stripe,......	150
4. Greenish slaty rock, ...	50
5. Dull red sandstone and conglomerate,......	100
6. A continued alternation of beds similar to 3, 4, and 5, for a thickness of at least 500 or	600
	1700

These gritstones and sandstones are generally hard and intractable, having a dull fracture, and being not well adapted for building purposes. The slaty beds are siliceous and the slaty cleavage imperfectly developed, the whole series being characterised by as few features of interest as can well be imagined. It seems somewhat to change in the nature of conglomerate beds in some places, as great masses of a grey colour, with small red pebbles imbedded, were observed, belonging apparently to this formation. As we descend to its lower beds, moreover, the quantity of the slaty rock increases. From these two circumstances, it may happen that the Trinity Bay sandstone-formation may be identical with the rocks I shall mention next. As, however, there is no direct evidence, except mineral character, in favour of this supposition, and some circumstances seem to militate against it, I shall describe these rocks separately.

4. *The Signal Hill Sandstone and Conglomerate.*—This formation consists of a group of rocks generally of a dull red colour, very hard and intractable, and thick-bedded. Its upper portion is principally a coarse-grained sandstone, frequently containing beds of conglomerate of quartzose pebbles, some of which are as large as a man's fist. In the lower part, the conglomerate is generally smaller, and it is interstratified with masses of a very fine grained gritstone of a very light grey colour, hard and splintery, the beds of which are commonly very thick, and in a limited section scarcely discernible. This grey stone may be seen at Quidi Vidi, Signal Hill, and the base of the South-side Hill of St John's. It is there used as building stone, but, like the gritstone of the Bell Isle formation, its utility for that purpose chiefly depends on the direction of the joints which traverse it, as it is difficult to trim it into shape. From all parts of the formation large square blocks might be frequently obtained fit for the construction of piers and breakwaters, or for similar purposes. The thickness of the formation, or of that part of it exhibited near St John's, must be about 800 feet.

5. *The St John's Slate-formation.*—The gradation downwards of the Signal Hill sandstones into this formation is perfect. At their junction, beds of dull red and greenish fine-grained gritstone alternate with each other, passing upwards into a coarser red sandstone, and downwards into a compact greenish rock, that gradually acquires a slaty cleavage, and assumes

all the aspect of clay-slate. This slate-formation varies considerably in character in different beds, and it is possible that the beds themselves may vary in different portions of their course. They are sometimes very thin, and split easily along the lines of stratification ; in this case the cleavage is frequently absent, or, if present, its plane appears generally to coincide with that of the stratification. Other beds, again, are very thick, the marks of stratification being confined to those bands of colour technically called the stripe, and having a fine cleavage crossing them at various angles, and splitting them into large and excellent roofing slates. The colour of these rocks varies from a greenish hue to a dark blue, or that which is commonly understood by slate colour. The thickness of the whole formation cannot be ascertained, as I do not know that I have anywhere seen the base of it ; that part which is exposed, however, must be 2000 or 3000 feet thick. It is the lowest stratified rock anywhere to be seen on the eastern side of the island.

Stratified Rocks of the Western part of Newfoundland.

The series of stratified rocks on the Western shore of Newfoundland is very different from that of the Eastern side. It consists of four or five formations in the following order :—

1. *The Newfoundland Coal-formation.*—This interesting and important group of rocks resembles in its higher portions the coal-formation of Europe, and consists of alternations of shale and clunch, with various beds of gritstone, and here and there a bed of coal. Interstratified with these rocks, however, there occur in Newfoundland beds of red marl ; and as we descend to the lower parts of the formation, there come in alternations of red and variegated marls with gypsum, dark blue clays with selenite, dark brown conglomerate beds, and soft and red and white sandstones. This inferior portion of the Newfoundland coal-formation so greatly resembles the new red sandstone of England (which in that country lies over the coal-formation), that it was not till I got the clearest evidence of the contrary, that I could divest myself of the prepossession of its being superior to the coal in this country also. That nothing might be wanting to complete the resemblance, a brine spring is known to rise in one spot on the south side of St

George's Bay, through the beds of red marl and sandstone. It is certain, however, that, in Newfoundland, the beds containing coal are above these red marls and sandstones, with gypsum and salt springs, the whole composing but one formation, which it is impossible to subdivide by any but the most arbitrary line of separation. The total thickness of this formation must be very considerable: I by no means have any reason to suppose that I have as yet seen its highest beds, while the thickness which I have seen must amount altogether to at least 1000 or 2000 feet.

The group of rocks which I believe to be next below the coal formation, is one that I shall call—

2. *The Port au Port Shale and Gritstone.*—This is a very large formation, something similar in character to that which, on the eastern side of the island, I have called the Belle Isle shale and gritstone; and it is perfectly possible that the two may be different portions of the same beds. The Port au Port beds, however, are not so regularly bedded as those of Belle Isle, the shales are less micaceous and more sandy, and many of the gritstone beds are laminated and schistose. The total thickness of the beds seen must exceed 1500 feet.

3. *The Humber Limestone.*—This group of rocks lies below the Port au Port shales and gritstones, and in the Bay of Islands it is the one *next* inferior; I cannot say whether the one graduates into the other, or whether other beds may not be interposed between the two in other localities. The highest part of the Humber Limestone which was visible, was a thin bedded mass, about thirty feet thick, of a hard slaty limestone of a dark grey colour, with brown concretions that, on a surface which had been sometimes exposed, stood out in relief. Below this are some beds of hard subcrystalline limestone, the colours of which are white or flesh coloured with white veins. These would take a good polish and would make very ornamental marbles, and from the thinness of the beds, are especially adapted for marble slabs. This series of beds has a thickness of about 200 feet. Below these are a few feet of similar beds of black marble, which rest on some grey compact limestone, with bands or thin beds, and irregular nodules of white chert; and these latter beds pass down into a large mass of similar limestone, without chert, and in very thick beds. This mass

of rock forms hills four or five hundred feet high, in nearly horizontal beds. Its upper part continues to be regularly bedded, but in its lower portion all distinction into beds is lost, and the limestone becomes perfectly white and saccharine. This great mass of white marble is frequently crossed by grey veins, so that I cannot say that I saw any block pure enough for the statuary. There is little doubt, however, that, in so large a quantity, some portions might be discovered fit for statuary marble; and for all other purposes to which marble is applied, the store is inexhaustible.

On the north side of St George's Bay, there is a limestone-formation, which I believe to be in the same situation as the Humber limestone, with respect to the Port au Port shale and gritstone; but which differs in character so very much from any beds I saw on the Humber, that I forbear to class it with that rock without further evidence. It consists principally of a light yellow magnesian limestone, having, however, interstratified beds of grey carbonate of lime. The grey beds frequently contain bands and nodules of chert or calcedony, and the yellow magnesian beds are frequently marked with light red concentric rings, which are sections of spheroidal bands of colour, but which do not appear to differ, except in colour, from the rest of the mass. These bands or rings are of rather irregular form, something resembling the bands of colour in a fortification agate, but being frequently three feet in diameter. The following is the most complete section I could get; but as it was cut off by the sea below, and concealed by the woods above, it affords no criterion as to the total thickness of the formation.

	Feet.
Thick-bedded light yellow magnesian limestone, ...	15
Thin-bedded ditto, with horizontal pink stripes, and having partings of indurated marl,..	8
Thick-bedded light yellow magnesian limestone, ...	10
Thin-bedded pinkish-yellow ditto with light red concentric rings,	20
Light grey limestone with a band of chert,.............................	5
Yellow magnesian limestone,..	2
	60

4. *Mica-slate and Gneiss.*—This formation, in whatever country it appears, is the lowest of the stratified rocks. Mica-slate is a laminated rock, made up of flakes of mica. Gneiss may

be described as stratified granite. As is often the case, they have in this country chlorite-slate and quartz-rock associated with them. The description of these rocks to be found in any elementary work on Geology, will equally apply to those of this country.

I have not made any mention of the igneous or unstratified rocks in the above summary. Those met with in the course of the above survey are basalt, greenstone, porphyry, hypersthene, syenite, and granite. It is of course entirely foreign to my plan to enter on a description of these rocks, as their characters are constant in all countries. Any remarkable varieties in them will be noticed in treating of the several localities in which they were found.—*(To be continued.)*

Comparative View of the Skulls of the various Aboriginal Nations of North and South America. By S. G. MORTON, Professor of Anatomy at Philadelphia.*

Dr Morton (formerly a pupil of our University), who has been for years actively employed in investigating the natural history of the native inhabitants of the New World, we rejoice to learn, has just published the results of his labours in this very interesting field of science, in a work which affords ample proof of his zeal, learning, and acuteness. As no copies of the "*Comparative View*" have as yet reached this country, we now submit to our readers, and nearly in its original form, an ample and judicious account of it, just published in the 78th Number of Silliman's American Journal of Science and Art.

The principal design of the work, says Dr Morton, has been, "to give accurate delineations of the crania of more than forty

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* The following is the full title of the work of Dr Morton :—"Crania Americana; or a Comparative View of the Skulls of various Aboriginal Nations of North and South America; to which is prefixed an Essay on the varieties of the Human Species; illustrated by seventy-eight plates, and a coloured map. By Samuel George Morton, M.D., Professor of Anatomy in Pennsylvania College at Philadelphia." Philadelphia: J. Dobson. London: Simpkin, Marshall, and Co. Pp. 296, folio, 1839.

Indian nations, Peruvian, Brazilian, and Mexican, together with a particularly extended series from North America, from the Pacific Ocean to the Atlantic, and from Florida to the region of the Polar tribes. Especial attention has also been given to the singular distortions of the skull caused by mechanical contrivances in use among various nations, Peruvians, Charibs, Natches, and the tribes inhabiting the Oregon Territory." His materials in this department, are so ample, that he has been enabled to give a full exposition of the subject. He has also bestowed particular attention on the crania from the mounds of this country, which have been compared with similar relics, derived both from ancient and modern tribes, "in order to examine by the evidence of osteological facts, whether the American aborigines, of all epochs, have belonged to one race, or to a plurality of races."

The introductory Essay, "on the varieties of the human species," occupies ninety-five pages. It is learned, lucid, and, like the whole work, classically written. The author notices the great diversities of opinion that have existed among naturalists regarding the grouping of mankind into races; Linnæus referred all the human family to five races; Buffon proposed six great divisions; subsequently, however, he reduced it to five; while Blumenbach, adopting the arrangement of Buffon, has changed the names of some of the divisions, and designated, with greater accuracy, their geographical distribution. Cuvier admitted three races only, the Caucasian, Mongolian, and Ethiopian.

Dr Morton adopts the arrangement of Blumenbach in so far as regards the great divisions, substituting, however, the word *race* for the term "variety" of the German author, and changing the order in which Blumenbach considers some of them. He considers the human species as consisting of *twenty-two* families, which he arranges under the heads of the Caucasian, Mongolian, Malay, American, and Ethiopian races.

I. "The CAUCASIAN RACE is characterised by a naturally fair skin, susceptible of every tint; hair fine, long and curling, and of various colours. The skull is large and oval, and its anterior portion full and elevated. The face is small in proportion to the head, of an oval form, with well proportioned features. The nasal bones are arched, the chin full, and the teeth vertical. The race is distinguished for the facility with which it attains the highest intellectual endowments."

The subdivisions of this race are into—1*st*, The *Caucasian;* 2*d*, The *Germanic;* 3*d*, The *Celtic;* 4*th*, The *Arabian;* 5*th*, The *Lybian;* 6*th*, The *Nilotic*, (Egyptian); and 7*th*, The *Indostanic* families.

II. "The MONGOLIAN RACE. This is characterized by a sallow or olive coloured skin,* which appears to be drawn tight over the bones of the face; long, black, straight hair, and thin beard. The nose is broad and short; the eyes are small, black and obliquely placed, and the eyebrows arched and linear; the lips are turned, the cheek bones broad and flat, and the zygomatic arches salient. The skull is oblong-oval, somewhat flattened at the sides, with a low forehead. In their intellectual character the Mongolians are ingenious, imitative, and highly susceptible of cultivation."

The subordinate divisions are into—8*th*, The *Mongol-Tartar;* 9*th*, The *Turkish;*† 10*th*, The *Chinese*; 11*th*, The *Indo-Chinese;* and 12*th*, The *Polar* families.

III. "The MALAY RACE. It is characterized by a dark complexion varying from a tawny hue to a very dark brown. Their hair is black, coarse, and lank, and their eyelids are drawn obliquely upwards at the outer angles. The mouth and lips are large, and the nose is short and broad, and apparently broken at its root. The face is flat and expanded, the upper jaw projecting, and the teeth salient. The skull is high and squared or rounded, and the forehead low and broad. This race is active and ingenious, and possesses all the habits of a migratory, predaceous, and maritime people."

The subdivisions embrace—13*th*, The *Malay;* and 14*th* The *Polynesian* (or South Sea Island) families.

IV. "The AMERICAN RACE is marked by a brown complexion, long, black, lank hair, and deficient beard.‡ The eyes are black and deep set, the brow low, the cheek bones high, the nose large and aquiline, the mouth large, and the lips tumid and compressed. The skull is small, wide between the parietal protuberances, prominent at the vertex, and flat on the occiput. In their mental character the Americans are averse to cultivation, and slow in acquiring knowledge; restless, revengeful, and fond of war, and wholly destitute of maritime adventure."

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* The olive colour does not occur in the skin of any of the races.—EDIT.

† Cuvier places the Turkish family in the Caucasian race, which is a preferable arrangement.—EDIT.

‡ The colours of the Malay and American races, as given by our author, are far from being correct, owing evidently to the employment of a faulty colour-system.—EDIT.

The families into which this race is subdivided, are two; 15*th*, The *American;* and 16*th*, The *Toltecan.*

V. "The ETHIOPIAN RACE is characterized by a black complexion, and black, woolly hair. The eyes are large and prominent, the nose broad and flat, lips thick, and the mouth wide. The head long and narrow, the forehead low, the cheek bones prominent, the jaws projecting and the chin small. In disposition, the Negro is joyous, flexible, and indolent; while the many nations which compose this race present a singular diversity of intellectual character, of which the far extreme is the lowest grade of humanity.

This race is divided into—17*th*, The *Negro;* 18*th*, The *Caffrarian;* 19*th*, The *Hottentot;* 20*th*, The *Oceanic Negro;* 21*st*, The *Australian;* and 22*d*, The *Alforian* families. The latter family is most numerous in New Guiana, the Moluccas and Magindano.

Dr Morton gives a brief but clear description, extending to his 91st page, of the leading characteristics of each of these families, accompanying his text by references to the authorities from which the information is drawn. The labour and accuracy of the true philosopher are here conspicuous. After perusing these details, however, we are strongly impressed with the conviction that this branch of science is still only in its infancy. The descriptions of the mental faculties which distinguish the different families of mankind, given even by the best travellers, are vague and entirely popular. There is scarcely an instance of the specification of well defined mental faculties, present or absent in the races, or possessed in peculiar combinations; nothing, in short, which indicates that the travellers possessed a mental philosophy, under the different heads of which they could classify and particularize the characteristic *qualities of* mind which they observed, as the botanists describe and classify plants, or the mineralogists minerals. The anatomical characters of the races, also, are still confined to a few particulars, and many even of these have been drawn from the inspection of a very limited number of specimens. The subject, however, possesses so much inherent interest and importance, that we may expect rapid advances to be made in its future development.

The unity of the human species is assumed by Dr Morton. It is known that the *black* race possess an apparatus in the skin, which is wanting in that of the *white* race. Flourens states, that there "are, in the skin of the *white* race, three distinct laminæ or membranes—the *derm*, and two *epiderms;* and in the skin of the *black* race, there is, besides the *derm* and the two epiderms of the *white* race, a particular apparatus, an apparatus which is altogether wanting in the man of the *white* race, an apparatus composed of two layers, the external of which is the seat of the *pigmentum*, or colouring matter of negroes."* "The colouring apparatus of the negro is always found in the mulatto." Flourens adds, "The *white* race and the *black* race are then, I repeat, two essentially distinct races. The same is true of the *red*, or American race. Anatomy discovers, under the second epiderm of the individual of the *red*, *copper-coloured*, *Indian*, or *American* race (for this race is called indifferently by all these names), a *pigmental apparatus*, which is the seat of the *red* or *copper colour* of this race, as the *pigmental apparatus* of the negro is the seat of his black colour."

Dr Morton does not advert to the existence of this pigmental apparatus in the American race. The investigations of Dr M'Culloh, he observes, "satisfactorily prove that the designation '*copper-coloured*,' is wholly inapplicable to the Americans as a race." "The *cinnamon* is, in Dr M'Culloh's apprehension, the nearest approach to the true colour" of the native Americans. Dr Morton considers that the "*brown race*" most correctly designates them collectively. "Although," says he, "the Americans thus possess a pervading and characteristic complexion, there are occasional and very remarkable deviations, including all the tints from a decided white to an unequivocally black skin." He shews, also, by numerous authorities, that "climate exerts a subordinate agency in producing these diversified hues." The tribes which wander along the burning plains of the equinoctial region, have no darker skins than the mountaineers of the temperate zone. "Again, the Puelchés, and other inhabitants of the Magellanic region, beyond the 55th degree of south latitude, are absolutely darker than the Abipones, Macobios, and Tobas, who are many degrees nearer the equator. While the Botocudys are of a clear brown colour, and sometimes nearly white, at no great distance from the tropic; and, moreover, while the Guyacas, under the line, are characterized by a fair complexion, the Charruas, who are almost black, inhabit the 50th degree of south latitude; and the yet blacker Californians are 25 degrees north of the equator." "After all," he adds, "these differences in complexion are extremely partial, forming mere exceptions to the primitive and national tint that characterizes these people from Cape Horn to the Canadas. The cause of these anomalies is not readily explained; that it is not climate, is sufficiently obvious; and whether it arises from partial immigrations from other countries, remains yet to be decided."

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* Annales des Sciences Nat. t. x. Dec. 1838, pp. 361, &c. Edinburgh New Philosophical *Journal*, vol. xxvii, p. 353.

Buffon defines species—"A succession of similar individuals which reproduce each other." Cuvier also defines species—"The union of individuals descended from each other or from common parents, and of those who resemble them as much as they resemble each other." "The apparent differences of the races of our domestic species," says Cuvier, "are stronger than those of any species of the same genus." "The fact of the *succession*, therefore, and of the *constant succession*, constitutes alone *the unity of the species*." Flourens, who cites these definitions, concludes that "*unity*, *absolute unity*, of the human species, and *variety* of its races, as a final result, is the general and certain conclusion of all the facts acquired concerning the *natural history of man*."*

Dr Morton, while he assumes the unity of the species, conceives that "each race was adapted from the beginning (by an all-wise Providence) to its peculiar local destination. In other words, that the physical characteristics which distinguish the different races, are independent of external causes."

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* *Flourens's* article before cited, and the Edinburgh New Philosophical *Journal, vol.* xxvii. p. 358, October 1839.

Dr Morton describes the general characteristics of the American, under the head of the "Varieties of the Human Species," and then enters on a special description of the "crania" of upwards of seventy nations or tribes belonging to that family, illustrating the text by admirable plates of the crania, drawn from skulls, mostly in his own possession, and of the full size of nature.

He regards the American race as possessing certain physical traits that serve to identify them in localities the most remote from each other. There are, also, in their multitudinous languages, the traces of a common origin. He divides the race into the "Toltecan family," which bears evidence of centuries of demi-civilization, and into the "American family," which embraces all the barbarous nations of the new world, excepting the Polar tribes, or Mongol Americans. The Eskimaux, and especially the Greenlanders, are regarded as a partially mixed race, among whom the physical character of the Mongolian predominates, while their language presents obvious analogies to that of the Chippewyans, who border on them to the south.

In the American family itself, there are several subordinate groups; 1*st*, The *Appalachian* branch includes all the nations of North America, excepting the Mexicans, together with the tribes north of the river of Amazon and east of the Andes. 2*d*, The *Brazilian* branch is spread over a great part of South America east of the Andes, viz., between the rivers Amazon and La Plata, and between the Andes and the Atlantic, thus including the whole of Brazil and Paraguay north of the 35th degree of south latitude. In character, these nations are warlike, cruel, and unforgiving. They turn with aversion from the restraints of civilized life, and have made but trifling progress in mental culture or the useful arts. In character, the Brazilian nations scarcely differ from the Appalachian; none of the American tribes are less susceptible of cultivation than these; and what they are taught by compulsion, in the missions, seldom exceeds the humblest elements of knowledge. 3*d*, The *Patagonian* branch includes the nations south of the La Plata, to the Straits of Magellan, and the mountain tribes of Chili. They are for the most part distinguished for their tall stature, their fine forms, and their indomitable courage, of all which traits the Auracanians possess a conspicuous share. 4*th*, The *Fuegian* branch, which roves over a sterile waste, computed to be as large as one-half of Ireland. Forster computes their whole number at only two thousand souls. Their physical aspect is altogether repulsive, and their domestic usages tend to heighten the defects of nature. The expression of the face is vacant, and their mental operations are to the last degree slow and stupid. The difference between them and the other Americans is attributed by Dr Morton to the effects of climate and locality.

Thus far, Dr Morton has travelled over ground previously occupied by other naturalists; but we proceed to a field in which he has had the courage and sagacity to enter boldly on a new path. He has added to his text numerous and minute measurements of the size and capacity not only of each entire cranium, but of its different parts, with a view to elucidate the connection (if there be any) between particular regions of the brain and particular mental qualities of the American tribes. In his dedication to John S. Phillips, Esq., of Philadelphia,* he observes: " It may, perhaps, be thought by some readers, that these details are unnecessarily minute, especially in the phrenological tables; and again, others would have preferred a work conducted throughout on phrenological principles. In this study I am yet a learner; and it appeared to me the wiser plan to present the facts unbiassed by theory, and let the reader draw his own conclusions. You and I have long admitted the fundamental principles of phrenology, viz., that the brain is the organ of the mind, and that its different parts perform different functions; but we have been slow to acknowledge the details of cranioscopy as taught by Dr Gall, and supported and extended by subsequent observers. We have not, however, neglected this branch of inquiry, but have endeavoured to examine it in connection with numerous facts, which can only be fully appreciated when they come to be compared with similar measurements derived from the other races of men." We shall state, in a subsequent part of this article, the conclusions at which Dr Morton has arrived, in consequence of his observations and measurements; meantime it is important to state the principles on which he proceeded.

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* Dr Morton acknowledges himself to be under many obligations to Mr Phillips in the prosecution of his inquiries, and says that it was he who invented the machines used in making the measurements, and executed many of them himself.

In a few years, it will appear a singular fact in the history of mind, that in the nineteenth century, men holding the eminent station in literature occupied by Lord Jeffrey and Lord Brougham, should have seriously denied* that the mind, in this world, acts by means of material organs; yet such is the case; and the denial can be accounted for only by that entire neglect of physiology, as a branch of general education, which prevailed in the last century, and by the fact that the metaphysical philosophy in which they were instructed, bore no reference to the functions of the brain. We need not say, that no adequately instructed naturalist doubts that the brain is the organ of the mind. But there are two questions, on which great difference of opinion continues to prevail; 1*st*, Whether the *size* of the brain (health, age, and constitution being equal) has any, and if so, what influence, on the power of mental manifestation? And, 2*dly*, Whether different faculties be, or be not, manifested by particular portions of the brain.

The *first* proposition, that the size of the brain, other conditions being equal, is in direct relation to the power of mental manifestation, is supported by analogy, by several well known facts, and by high physiological authorities. The power of smell, for example, is great in proportion to the expansion of the olfactory nerve on the internal nostrils, and the volume of the nerve itself bears a direct relation to the degree of that expansion. The superficial surface of the mucous membrane of the ethmoidal bone, on which the nerve of smell is ramified, is computed in man to extend to 20 square inches, and in the seal, which has great power of smell, to 120 square inches.

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* Lord Jeffrey, in the Edin. Review, No. 88, and Lord Brougham in his Discourse on Natural Theology, p. 120.

The optic nerve in the mole is a slender thread, and its vision is feeble; the same nerve is large and thick in the eagle, accompanied by intense powers of sight. Again, the fact admits of demonstration, that deficiency in the size of the brain is one, although not the only, cause of idiocy. Although the brain be healthy, if the horizontal circumference of the head, with the muscular integuments, do not exceed thirteen or fourteen inches, idiocy is the *invariable* consequence. Dr Voisin states that he made observations on the idiots under his care at the Parisian Hospital of Incurables, and found that in the lowest class of idiots, where the intellectual manifestations were null, the horizontal circumference, taken a little higher than the orbit, varied from eleven to thirteen inches, while the distance from the root of the nose backwards, over the top of the head, to the occipital spine, was only between eight and nine inches; and he found no exception to this fact. If, therefore, extreme defect of size in the brain be invariably accompanied by mental imbecility, it is a legitimate inference that size will influence the power of manifestation through all other gradations of magnitude, always assuming other conditions to be equal.

Physiological authorities are equally explicit on this subject. Magendie says, "the volume of the brain is generally in direct proportion to the capacity of the mind. We ought not to suppose, however, that every man having a large head is necessarily a person of superior intelligence; for there are many causes of an augmentation of the volume of the head besides the size of the brain; but it is rarely found that a man distinguished by his mental faculties has not a large head. The only way of estimating the volume of the brain, in a living person, is to measure the dimensions of the skull; every other means, even that proposed by Camper, is uncertain."

The difference of mental power between young and adult minds, is a matter of common observation. The difference in the weights of their brains is equally decided.

According to Cruveilhier, in three young subjects, the weights of the brains were as follows:

In the first, the brain weighed 2 lb. 2 oz.; the cerebellum, 4½ oz.; together, 2 lb. 6½ oz. In the second, the brain weighed

2 lb. 8 oz.; the cerebellum, 3½ oz.; together, 2 lb. 11½ oz. In the third, the brain weighed 2 lb. 5 oz.; the cerebellum, 5 oz.; together, 2 lb. 10 oz.

In the appendix to Dr Monro's work on the brain, Sir William Hamilton states the average weight of the *adult* male Scotch brain and cerebellum to be 3 lb. 8 oz. troy.

Again, a difference in mental power between men and women is also generally admitted to exist, and there is a corresponding difference in the size of their brains.

Sir William Hamilton states the average weight of the adult female Scotch brain and cerebellum, to be 3 lb. 4 oz. troy; being 4 oz. less than that of the male. He found one male brain in *seven* to weigh above 4 lbs.; and only one female brain in a *hundred* exceeded this weight.

In an essay " on the brain of the negro, compared with that of the European and the ourang outang," published in the Philosophical Transactions for 1836, part 2, Professor Tiedemann, of Heidelberg, adopts the same principle. After mentioning the weights of fifty-two European brains, examined by himself, he states that " the weight of the brain in an adult male European, varies between 3 lb. 2 oz. and 4 lb. 6 oz. troy. The brain of men who have distinguished themselves by their great talents, is often very large. The brain of the celebrated Cuvier weighed 4 lb. 11 oz. 4 dr. 30 gr. troy, and that of the distinguished surgeon Dupuytren weighed 4 lb. 10 oz. troy. The brain of men endowed with but feeble intellectual powers is, on the contrary, often very small, particularly in congenital idiotismus. The female brain is lighter than that of the male. It varies between 2 lb. 8 oz. and 3 lb. 11 oz. I never found a female brain that weighed 4 lb. The female brain weighs on an average from four to eight ounces less than that of the male; and the difference is already perceptible in a new-born child."

We have adduced these proofs and authorities in support of the proposition that size influences power, because we conceive it to be a principle of fundamental importance in every investigation into the natural history of man, founded on the physiology of the brain. One of the most singular features in the history of this continent, is, that the aboriginal races, with few exceptions, have perished or constantly receded, before the Anglo-Saxon race, and have in no instance either mingled with them as equals, or adopted their manners and civilization. These phenomena must have a cause; and can any inquiry be at once more interesting and philosophical than that which endeavours to ascertain whether that cause be connected with a difference in the brain between the native American race, and their conquering invaders? Farther, some few of the American families, the Auracanian, for instance, have successfully resisted the Europeans; and the question is important, whether in them, the brain be in any respect superior to what it is in the tribes which have unsuccessfully resisted?

It is true, that Dr Gall's fundamental principle, that size in the brain (other conditions being equal) is a measure of the power of mental manifestation, is directly involved in these inquiries; but we can discover no reason why it should not be put to the test of an extensive and accurate induction of facts.

Dr Morton reports the size in cubic inches, of the interior of nearly every skull described by him. "An ingenious mode," says he, "of taking the measurement of the internal capacity, was devised by Mr Phillips. In order to measure the capacity of a cranium, the foramina were first stopped with cotton, and the cavity was then filled with *white pepper seed*,* poured into the foramen magnum until it reached the surface, and pressed down with the finger until the skull would receive no more. The contents were then transferred to a tin cylinder, which was well shaken in order to pack the seed. A mahogany rod (previously graduated to denote the cubic inches and parts contained in the cylinder) being then dropt down, with its foot resting on the seed, the capacity of the cranium, in cubic inches, is at once read off on it."

Dr Morton gives also measurements of particular regions of the brain, as indicated by the skull.

Secondly. The most distinguished philosophers on the mind, divide the human faculties into the active and intellectual powers; and some admit even subdivisions of the feelings into propensities common to man with the lower animals, and moral emotions; and of the intellect, into observing and reflecting faculties. Dr Thomas Brown's division of the intellectual powers into simple and relative suggestion, corresponds with this last classification. If, then, the mind manifest a plurality of faculties, and if the brain be the organ of the mind, it appears to be a sound inference that the brain *may* consist of a plurality of organs. The presumptions which arise, in favour of this idea, from the constitution of the external senses and their organs are strong. Each sense has its separate nervous apparatus. Nay, when the function of a part is compound, the nerves are multiplied, so as to give a distinct nerve for each function. The tongue has a nerve for voluntary motion, another for common sensation, and the best authorities admit a third nerve for *taste*, although the precise nerve is still in dispute. The internal nostrils are supplied with two nerves, the olfactory, and a nerve of common sensation, ramified on the mucous membrane, each performing its appropriate function. The spinal marrow consists, by general consent of physiologists, of at least two double columns, the anterior pair for voluntary motion, and the posterior pair for common sensation. Sir Charles Bell has demonstrated the distinct functions of the nerves proceeding from these columns. Farther, every accurate observer distinguishes diversities of disposition and inequalities of talents, even in the same individual. The records of lunatic asylums shew numerous instances of partial idiocy and partial insanity. These facts indicate that the brain consists of a plurality of organs, and this idea is countenanced by many high authorities in physiological science. "The brain is a very complicated organ," says Bonnet, "or rather *an assemblage of very different organs*."* Tissot contends that every perception has different fibres;† and Haller and Van Swieton were of opinion that the internal senses occupy, in the brain, organs as distinct as the nerves of the external senses.‡ Cabanis entertained a similar notion,§ and so did Prochaska. Cuvier says, that "*certain parts* of the brain, in all classes of animals, are *large or small, according to certain qualities of the animals*;"* and he admits that Gall's doctrine of different faculties being connected with different parts of the brain, is nowise contradictory to the general principles of physiology.†

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* "White pepper seed was selected on account of its spherical form, its hardness, and the equal size of the grains. It was also sifted, to render the equality still greater."

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* Palingénésie, 1334. † Œuvres, III. 33. ‡ Van Swieten, I. 454.

§ Rapports du Physique et du Moral de l'Homme, 2de Edit. I. 233-4.

If, then, there be reason to believe that different parts of the brain manifest different mental faculties, and if the size of the part influence the power of manifestation, the necessity is very evident of taking into consideration the *relative proportions of the different parts of the brain*, in a physiological inquiry into the connection between the crania of nations and their mental qualities. This position is further illustrated by the fact, that the native American and the European brains *differ widely in the proportions of their different parts.*

We have entered more minutely into the reasons why we regard these measurements as important, because we conceive that the distinguishing excellence of Dr Morton's work consists in his having adopted and followed out this great principle. It appeared necessary to dwell upon it at some length, also, because Professor Tiedemann, in his comparison of the European with the Negro brain, has entirely neglected it, and in consequence has arrived at physiological conclusions which we regard as at variance with the most certain psychological facts, viz., he says, that "there is undoubtedly a very close connection between the ABSOLUTE SIZE of the brain and the INTELLECTUAL POWERS AND FUNCTIONS of the mind;" and proceeding on this principle, he compares the weight of the whole brain, as ascertained in upwards of fifty Europeans of different ages and countries, with its weight in several Negroes, examined either by himself or others. He gives extensive tables shewing the weight of the quantity of millet seed necessary to fill Ethiopian, Caucasian, Mongolian, American, and Malay skulls; and adds that "the cavity of the skull of the Negro in general, *is not smaller* than that of the European and other human races." The inference which he draws is, that, *intellectually* and *morally*, as well as anatomically, the Negro is naturally on a par with the European ; and he contends that the opposite and popular notion is the result of superficial observation, and is true only of certain degraded tribes on the *coast* of Africa.*

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* Anatomie comparée, tome ii.

† Rapport Historique sur les Progrès des Sciences Naturelles, &c. p. 193.

We entertain a great respect for Prof. Tiedemann, but we cannot subscribe to his principle that the whole brain is the measure of the *intellectual* faculties ; a proposition which assumes that the animal and moral feelings have no seat in this organ.

We shall now present a brief view of the manner in which Dr Morton applies his own principles, and of some of the conclusions at which he has arrived.

He divides the native American nations into two great families, the *Toltecan* and *American.* "It is in the intellectual faculties," says he, "that we discover the greatest difference between them. In the arts and sciences of the former we see the evidences of an advanced civilization. From the Rio Gila in California, to the southern extremity of Peru, their architectural remains are every where encountered to surprise the traveller and confound the antiquary ; among these are pyramids, temples, grottos, bas-reliefs, and arabesques ; while their roads, aqueducts, and fortifications, and the sites of their mining operations, sufficiently attest their attainments in the

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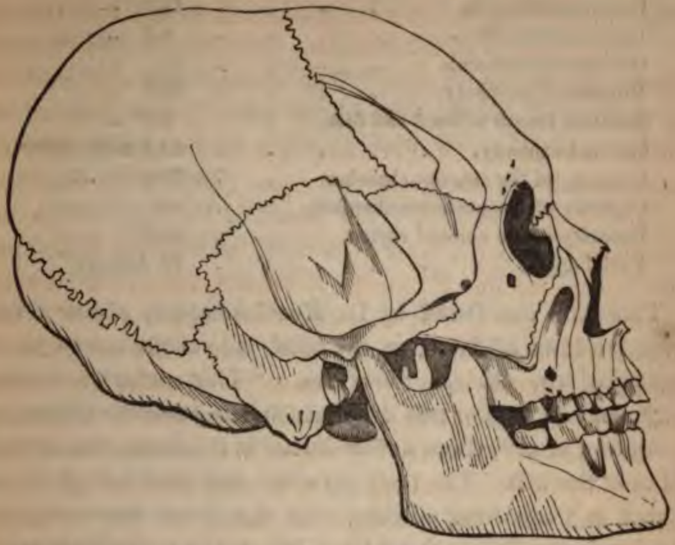
* Tiedemann's Essay has been critically examined by Dr A. Combe, in the Phrenological Journal, (vol. xi.) who shews not only the error of principle committed by the author in assuming the whole brain to be the organ exclusively of the *intellectual* faculties, but the more striking fact that Tiedemann's own tables refute his own conclusions. Tiedemann's measurements are the following :

	Inches.	Lines.
Average length of brain in 4 Negroes, . .	5	11
do. do. do. 7 European males, .	6	2⅐
do. do. do. 6 European females, .	5	10½
do. greatest breadth in 4 Negroes, . .	4	8⅙
do. do. do. 7 European males, .	5	1⅐
do. do. do. 3 European females, .	5	4⅓
do. height of brain in 3 Negroes, . .	2	11⅓
do. do. do. 7 European males, .	3	4
do. do. do. 4 European females, .	2	9½

The inferiority of the Negro brain in size, is self-evident from these dimensions.

practical arts of life." P. 84. The desert of Atacama divides the kingdom of Peru from that of Chilé, and is nearly a hundred miles in length. A river, abounding in salt, runs through it. This desert was the favourite sepulchre of the Peruvian nations for successive ages. The climate, salt, and sand, dry up the bodies, and the remains of whole generations of the former inhabitants of Peru may now be examined, after the lapse perhaps of thousands of years. Dr Morton has been enabled to examine nearly one hundred Peruvian crania, and concludes that that country has been, at different times, peopled by two nations of differently formed crania, one of which is perhaps extinct, or at least exists only as blended by adventitious circumstances, in very remote and scattered tribes of the present Indian race. "Of these two families, that which was antecedent to the appearance of the Incas is designated as the *ancient Peruvian*, of which the remains have been found only in Peru, and especially in that division of it now called Bolivia. Their tombs, according to Mr Pentland, abound on the shores and islands of the great Lake Titicaca, in the inter-alpine valley of the Desaquadera, and in the elevated valleys of the Peruvian Andes, between the latitudes of 14° and 19° 30′ south." Our knowledge of their physical appearance is derived solely from their tombs. They were not different "from cognate nations in any respect except in the conformation of the head, which is small, greatly elongated, narrow in its whole length, with a very retreating forehead, and possessing more symmetry than is usual in skulls of the American race. The face projects, the upper jaw is thrust forward, and the teeth are inclined outward. The orbits of the eyes are large and rounded, the nasal bones salient, the zygomatic arches expanded; and there is a remarkable simplicity in the sutures that connect the bones of the cranium." P. 97. Dr Morton presents the following cranium, Fig. 4 of his series, "as an illustrative type of the cranial peculiarities of the people;" and he is of opinion that the form is "natural, unaltered by art."

ANCIENT PERUVIAN, Fig. 4.



He gives the following description of this cranium :—

" Though the forehead retreats rapidly, there is but little expansion at the sides ; and from the face to the occiput, inclusive, there is a narrowness that seems characteristic of the race. The posterior view represents the skull elevated in that region, without any unnatural width at the sides, and the vertical view sufficiently confirms the latter fact.

*Measurements.**

Longitudinal diameter, ...	7.3 inches.
Parietal do.	5.3 ...

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* The measurements are thus described by Dr Morton. The *longitudinal* diameter is taken from the most prominent part of the os frontis to the occiput; the *parietal* diameter from the most distant points of the parietal bones ; the *frontal* diameter from the anterior-inferior angles of the parietal bones ; the *vertical* diameter from the fossa, between the condyles of the occipital bone, to the top of the skull ; the *inter-mastoid arch* is measured with a graduated tape, from the point of one mastoid process to the other, over the external tables of the skull ; the *inter-mastoid* line is the distance, in a straight line, between the points of the mastoid processes; the *occipito-frontal arch* is measured by a tape over the surface of the cranium, from

Frontal diameter,	4.3 inches.
Vertical do.	5.3 ...
Inter-mastoid arch,	14. ...
Inter-mastoid line,	4.3 ...
Occipito-frontal arch,	15. ...
Horizontal periphery,	19.8 ...
Extreme length of head and face,	8.2 ...
Internal capacity,	81.5 cubic inches.
Capacity of the anterior chamber,	31.5 ...
Capacity of the posterior chamber,	50. ...
Capacity of the coronal region,	16.25 ...
Facial angle,	73 degrees."

This skull was found by Dr Ruschenberger, about a mile from the town of Arica, on the south side of the *morro*, a cemetery of the ancient Peruvians. "The surface is covered with sand an inch or two deep, which being removed discovers a stratum of salt, three or four inches in thickness, that spreads all over the hill. The body (to which this head belonged) was placed in a squatting posture, with the knees drawn up and the hands applied to the sides. The whole was enveloped in a coarse, but close fabric, with stripes of red, which has withstood, wonderfully, the destroying effects of ages, for these interments were made before the conquest, although at what period is unknown."

Dr Morton states that the average internal capacity of the Caucasian or European head is at least 90 cubic inches. In three adult ancient Peruvians, it is only 73. The mean capacity of the anterior chamber is about one-half of that of the posterior, or 25 to 47, while the mean facial angle is but 67 degrees.

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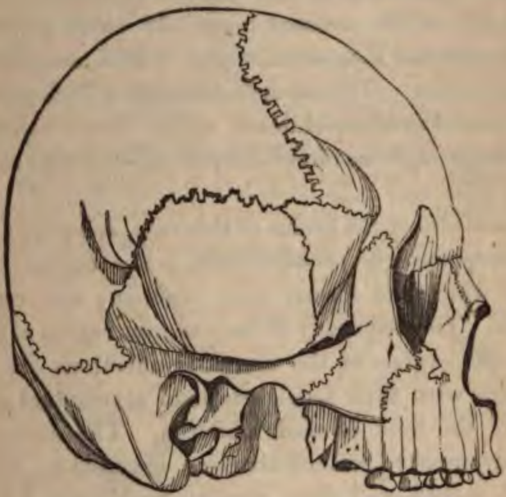
the posterior margin of the foramen magnum to the suture, which connects the os frontis with the bones of the nose; the *horizontal periphery* is measured by passing a tape round the cranium so as to touch the os frontis immediately above the superciliary ridges, and the most prominent part of the occipital bone: the *length of the head and face* is measured from the margin of the upper jaw, to the most distant point of the occiput; the *zygomati diameter* is the distance, in a right line, between the most prominent points of the *zygomæ;* the *facial angle* is ascertained by an instrument of ingenious construction and easy application, invented by Dr Turnpenny, and described by Dr Morton. Dr Morton took nearly all the anatomical measurements with his own hands.

"It would," he continues, "be natural to suppose, that a people with heads so small and badly formed, would occupy the lowest place in the scale of human intelligence. Such, however, was not the case." He considers it ascertained that "civilization existed in Peru anterior to the advent of the Incas, and that those anciently civilized people constituted the identical nations whose extraordinary skulls are the subjects of our present inquiry.

If these skulls had been compressed by art, we could have understood that certain portions of the brain might have been only displaced, but not destroyed. The spine, for instance, may be bent, as in hump-back, yet retain its functions; and we might suppose the anterior lobe, in cases of compression, to be developed laterally, or backwards, and still preserve its identity and uses. This, indeed, is Dr Morton's own conclusion, in regard to the brain in the flat-headed Indians. He gives an interesting and authentic description of the instrument and process by means of which the flat-head tribes of Columbia River compress the skull, and remarks, that "besides the depression of the head, the face is widened and projected forward by the process, so as materially to diminish the facial angle; the breadth between the parietal bones is greatly augmented, and a striking irregularity of the two sides of the cranium almost invariably follows; yet the absolute internal capacity of the skull is not diminished, and, strange as it may seem, the intellectual faculties suffer nothing. The latter fact is proved by the concurrent testimony of all travellers who have written on the subject." Dr Morton adds, that in January 1839, he was gratified with a personal interview with a full blood Chenouk, in Philadelphia. He is named William Brooks, was twenty years of age, had been three years in charge of some Christian missionaries, and had acquired great proficiency in the English language, which he understood and spoke with a good accent and general grammatical accuracy. His head was as much distorted by mechanical compression, as any skull of his tribe in Dr Morton's possession. "He appeared to me," he adds, "to possess more mental acuteness than any Indian I had seen, was communicative, cheerful, and well mannered." The measurements of his head were these: longitudinal diameter, 7.5 inches; parietal diameter' 6.9 inches; frontal diameter, 6.1 inches; breadth between the cheek bones, 6.1 inches; facial angle, about 73 degrees. Dr Morton considers it certain that the forms of the skull produced by compression, never become congenital, even in successive generations, but that the characteristic form is always preserved, unless art has directly interfered to distort it. Pp. 206, 207.

The extinct race in Peru, was succeeded by the "INCA, or MODERN PERUVIANS." This race dates its possession of Peru from about the eleventh century of our era; and as this period corresponds with the epoch of the migration from Mexico of the Toltecas, the most civilized nation of ancient Mexico, Dr Morton concurs in the opinion expressed by other authors, that the modern Peruvians were of a common origin with the ancient Mexicans. "The modern Peruvians," says he, "differ little in person from the Indians around them, being of the middling stature, well limbed, and with small feet and hands. Their faces are round, their eyes small, black, and rather distant from each other; their noses are small, the mouth somewhat large, and the teeth remarkably fine. Their complexion is a dark brown, and their hair long, black, and rather coarse." P. 115. The civilization and comparative refinement of the Incas was blended with some remains of the ferocity of the savage. "Matrimonial engagements were entered into with very little ceremony or forethought, and they were as readily set aside at the option of the parties. Polygamy was lawful but not prevalent." Among the people, incontinence, sensuality, and child-murder were common. Their diet was chiefly vegetables. The people were indolent, filthy, and negligent in their persons. The hair of their mummies, in many instances, is charged with desiccated vermin. Their religious system was marked by great simplicity, and was divested of those bloody rites which were common with the Aztecs of Mexico. They believed in one God, whom they called Viracocha, in the immortality of the soul, and in rewards and punishments in the next life. They worshipped both the sun and moon, in whose honour they erected temples and formed idols. They consecrated virgins, in the same manner as practised in modern convents. Their funeral rites were barbarous and cruel: when their chief men died, they buried a number of human victims, women, boys, and servants, to attend on the departed in the next world. They were conquered by Pizzaro with a force which consisted of 62 horsemen and 102 foot soldiers. P. 124. The following is given as a strikingly characteristic Peruvian head.

MODERN PERUVIAN, Fig. 5.



"The skull in these people," says Dr Morton, "is remarkable for its small size, and for its quadrangular form. The occiput is greatly compressed, sometimes absolutely vertical; the sides are swelled out, and the forehead is somewhat elevated, but very retreating. The skulls are remarkable for their irregularity. The dimensions of this skull are as follows;

Longitudinal diameter, . . .	6.1 inches.
Parietal do. . . .	6. ...
Frontal do. . . .	4.7 ...
Vertical do. . . .	5.5 ...
Inter-mastoid arch, . . .	16. ...
Inter-mastoid line, . . .	4.5 ...
Occipito-frontal arch, . . .	14.1 ...
Horizontal periphery, . . .	19.5 ...

Internal Capacity,	83.	cubic inches.
Capacity of the anterior chamber,	33.5	...
Capacity of the posterior chamber,	49.5	...
[*] Capacity of the coronal region,	15.75	..
Facial angle,	81	degrees."

Dr Morton gives the result of the measurement of twenty-three adult skulls of the pure Inca race. "The mean of the internal capacity is 73 cubic inches, which is probably lower than that of any other people now existing, not excepting the Hindoos." The mean of the anterior chamber is 32, of the posterior 42, of the coronal region 12 cubic inches. The highest measure of the coronal region is 20.5, and the smallest 9.25 cubic inches. The mean facial angle is 75 degrees. The heads of nine Peruvian children appear to be nearly, if not quite, as large as those of children of other nations at the same age. P. 133.

The small size of the brains of this race, compared with that of the Europeans who invaded them, is in accordance with the ease with which the former were overcome and retained in subjection. The deficiency in the posterior region of the brain, in which the organs of the domestic affections are situated, corresponds with their feeble conjugal attachment and indifference to the lives of their children. The diameter from constructiveness to constructiveness, is stated by Mr Phillips to be 4.5 inches, and from ideality to ideality 5.1. These organs give a talent for art, and are considerable. The same measurements in the Naumkeagh, the race which occupied New England, and whose skulls are still dug up near Boston and Salem, and which never made any attainments in the arts, are 4.1 and 4 inches, respectively. Dr Robertson, in his history of America, mentions that the modern Peruvian race was distinguished for its extraordinary powers of concealment and secrecy. Mr Phillips states the breadth from secretiveness to secretiveness to be 5.6 inches, which is large; the longitudinal diameter is only 6.1. The region of combativeness also appears to be deficient in these skulls.

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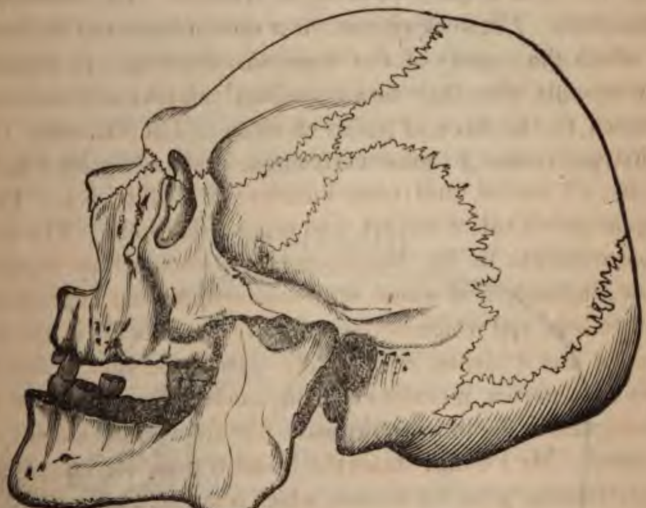
[*] The *chambers* and *coronal region* of the cranium are explained by a figure, which we regret the hurry of publication prevented us copying into our Journal.—Edit.

The Iroquois confederacy consisted originally of five nations, the Mohawks, Oneidas, Onondagas, Cayugas, and Senecas. They were intellectually superior to the surrounding nations, passionately devoted to war, and victorious over the other tribes. They forced their women to work in the field and carry burdens; they paid little respect to old age, were not much affected by love, were regardless of connubial obligations, and addicted to suicide. "They were proud, audacious, and vindictive, untiring in the pursuit of an enemy, and remorseless in the gratification of their revenge. Their religious ideas were vague, and their cautiousness and cunning proverbial. They were finally subdued and nearly exterminated by the Anglo-Americans in 1779. Some miserable remnants of them, ruined by intoxicating liquors, still exist in the state of New York." The following is the skull of a Huron, one of these nations.

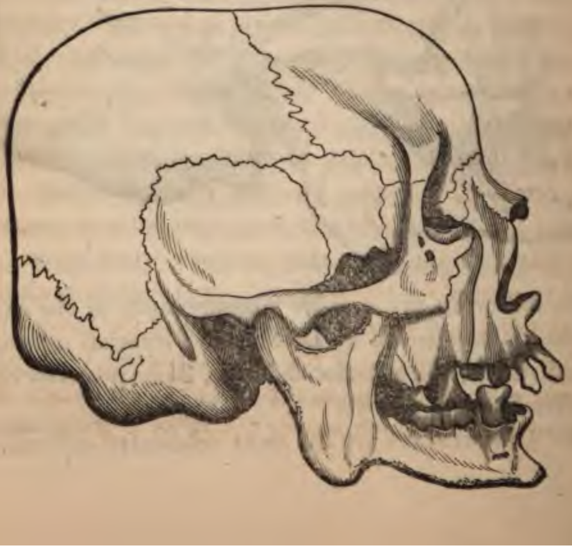
Huron, Fig. 6.



The following are *average* measurements of the five skulls of these nations given by Dr Morton: internal capacity, 88; coronal region, 15; anterior chamber, 31.5; posterior chamber, 50 cubic inches.

The Araucanians are the most celebrated and powerful of the Chilian tribes. They inhabit the region between the rivers Biobio and Valdivia, and between the Andes and the sea, and derive their name from the province of Arauco. "They are a robust and muscular people, of a lighter complexion than the surrounding tribes. Endowed with an extraordinary degree of bodily activity, they reach old age with few infirmities, and, generally, retain their sight, teeth, and memory, unimpaired. They are brave, discreet, and cunning to a proverb, patient in fatigue, enthusiastic in all their enterprises, and fond of war as the only source of distinction." "Their vigilance soon detected the value of the military discipline of the Spaniards, and especially the great importance of cavalry in an army; and they lost no time in adopting both these resources, to the dismay and discomfiture of their enemies. Thus in seventeen years after their first encounter with Europeans, they possessed several strong squadrons of horse, conducted their operations in military order, and, unlike the Americans generally, met their enemies in the open field." "They are highly susceptible of mental culture, but they despise the restraints of civilization, and those of them who have been educated in the Spanish colonies, have embraced the first opportunity to resume the haunts and habits of their nation." P. 241. The following is one of three Araucanian skulls delineated in the work.

ARAUCANIAN, Fig. 7.



The average measurements of the three skulls are as follows: internal capacity, 79; coronal region, 15.4; anterior chamber 32.2; posterior chamber, 48.50.

We have no space to enter into any description of the skulls found in the ancient tombs, or of those of the Flat-headed Indians and Charibs; suffice it to say, that Dr Morton's materials are full and satisfactory on these topics, and his facts and conclusions highly interesting. We subjoin a few of the general results at which he arrives from a survey of his entire field.

"The intellectual faculties," says he, "of the great AMERICAN FAMILY, appear to be of a decidedly inferior cast, when compared with those of the Caucasian or Mongolian races. They are not only averse to the restraints of education, but for the most part incapable of a continued process of reasoning on abstract subjects. Their minds seize with avidity on simple truths, while they at once reject whatever requires investigation and analysis. Their proximity, for more than two centuries, to European institutions, has made scarcely any appreciable change in their mode of thinking or their manner of life; and as to their own social condition, they are probably in most respects what they were at the primitive epoch of their existence. They have made few or no improvements in building their houses or their boats; their inventive and imitative faculties appear to be of a very humble grade, nor have they the smallest predilection for the arts or sciences. The long annals of missionary labour and private benefaction bestowed upon them, offer but very few exceptions to the preceding statement, which, on the contrary, is sustained by the combined testimony of almost all practical observers. Even in cases where they have received an ample education, and have remained for many years in civilized society, they lose none of their innate love of their own national usages, which they have almost invariably resumed when chance has left them to choose for themselves." "However much the benevolent mind may regret the inaptitude of the Indians for civilization, the affirmative of this question seems to be established beyond a doubt. His moral and physical nature are alike adapted to his position among the races of men; and it is as reasonable to expect the one to be changed as the other. The structure of his mind appears to be different from that of the white man; nor can the two harmonise in their social relations except on the most limited scale. Every one knows, however, that the mind expands by culture; nor can we yet tell how near the Indian would approach the Caucasian after education had been bestowed on a single family through several successive generations." P. 82.*

The following are parts of Dr Morton's table of "mean results," given from his whole measurements.

	Toltecan nations, and skulls from mounds.		Barbarous nations, with skulls from the valley of Ohio.		American race, embracing Toltecan and barbarous nations.		Flat head tribe of Columbia river.		Ancient Peruvians.	
	No. of skulls.	Mean:	No. of skulls.	Mean.	No. of skulls.	Mean.	No. of skulls.	Mean.	No. of skulls.	Mean.
Internal capacity in cubic inches,	57	76.8	87	82.4	144	79.6	8	79.25	3	73.2
Capacity of anterior chamber,	46	32.5	73	34.5	119	33.5	8	32.25	3	25.7
Capacity of posterior chamber,	46	43.8	73	48.6	119	46.2	8	47.00	3	47.4
Capacity of coronal region,	46	14.0	71	16.2	117	15.1	8	11.09	3	14.6
Capacity of sub-coronal region,	46	61.8	71	66.5	117	64.5	8	67.35	3	58.6

Remarks.—"The barbarous nations possess a larger brain by 5½ cubic inches, than the Toltecans; while, on the other hand, the Toltecans possess a greater relative capacity of the anterior chamber of the skull, in the proportion of 42.3 to 41.8. Again, the coronal region, though absolutely greater in the barbarous tribes, is rather larger in proportion in the demi-civilized tribes; and the facial angle is much the same in both, and may be assumed, for the race, at 75 degrees.

"In conclusion, the author is of the opinion that the facts contained in this work tend to sustain the following propositions:

"1*st*, That the American race differs essentially from all others, not excepting the Mongolians; nor do the feeble analogies of language, and the more obvious ones, in civil and religious institutions and the arts, denote any thing beyond casual or colonial communication with the Asiatic nations; and even those analogies may perhaps be accounted for, as Humboldt has suggested, in the mere coincidence arising from similar wants and impulses in nations inhabiting similar latitudes.

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* Dr Morton adds, that the Indians are extremely defective in comprehending every thing relating to numbers; and we may remark, that Mr Combe, in his lectures in New Haven, shewed the great deficiency of the organ of number in their skulls.

"2d, That the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character.

"3d, That the cranial remains discovered in the mounds from Peru to Wisconsin, belong to the same race, and probably to the Toltecan family." Dr Morton subjoins the following

"NOTE *on the internal capacity of the cranium in the different races of men.* Having subjected the skulls in my possession, and such also as I could obtain from my friends, to the internal capacity measurement already described, I have obtained the following results. The mean of the American race (omitting fractions) is repeated here, merely to complete the table. The skulls of idiots, and persons under age, were of course rejected.

Races.	No. of skulls.	Mean internal capacity in cubic inch.	Largest in the series.	Smallest in the series.
1. Caucasian,	52	87	109	75
2. Mongolian,	10	83	93	69
3. Malay,	18	81	89	64
4. American,	147	80	100	60
5. Ethiopian,	29	78	94	65

"1st. The *Caucasians* were, with a single exception, derived from the lowest and least educated class of society. It is proper, however, to mention that but three Hindoos are admitted in the whole number, because the skulls of these people are probably smaller than those of any other existing nation. For example, seventeen Hindoo heads give a mean of but seventy-five cubic inches; and the three received into the table are taken at that average. To be more specific, we will give, in detail, the number of individuals of each nation, as far as ascertained.

Anglo-Americans,	6
German, Swiss and Dutch,	7
Celtic, Irish, and Scots,	7
English,	4
Guanché (Lybian),	1
Spanish,	1
Hindoo,	3
Europeans not ascertained,	23
Total,	52

"2d. The *Mongolians* measured, consist of Chinese and Eskimaux, and what is worthy of remark, three of the latter give a mean of 86 cubic inches, while seven Chinese give but 82.

"3d. The Malays embrace Malays proper and Polynesians, thirteen of the former and five of the latter; and the mean of each presents but a fractional difference from the mean of all.

"4th. The ***Ethiopians*** were all unmixed negroes, and nine of them native Africans, for which I am chiefly indebted to Dr M'Dowell, formerly attached to the colony at Liberia.*

"5th. Respecting the American race, I have nothing to add, excepting the striking fact that, of all the American nations, the Peruvians had the smallest heads, while those of the Mexicans were something larger, and those of the barbarous tribes the largest of all, viz.

Toltecan nations,	Peruvians collectively,	76	cub. inch.
	Mexicans collectively,	79	do.
	Barbarous tribes, as per table,	82	do.

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* Dr Morton states the mean internal capacity of the European, or Caucasian skulls, to be 87 cubic inches, and of the Ethiopian, or Negro race, to be 78 cubic inches. We observe that Dr Andrew Combe, in his "Remarks on the Fallacy of Professor Tiedemann's Comparison of the Negro brain and intellect with those of the European," arrives at results coinciding with those obtained by Dr Morton. Tiedemann gives the weight of only four Negro brains. "The average European," he says, "runs from 3 lb. 2 oz. to 4 lb. 6 oz.; while the average of the four Negro brains rises to only 3 lb. 5 oz. 1 dr.; or 3 oz. above the lowest European averages; and the *highest* Negro falls 5 oz. short of the highest *average* European, and no less than 10 oz. short of Cuvier's brain." Phren. Journ., vol. xi. We have already shewn, p. 357, that Tiedemann's linear dimensions of the European and Negro brain also contradict his theory of equality, an are in harmony with Dr Morton's results.

"An interesting question remains to be solved, viz. the relative proportion of brain in the anterior and posterior chambers of the skull in the different races; an inquiry for which I have hitherto possessed neither sufficient leisure nor adequate materials." P. 261.

Our readers will discover in the length and minuteness of this article, the great value which we attach to Dr Morton's work. We regard it as an honour to the country, and as a proof of talent, patience, and research in himself, which place him in the first rank among natural philosophers. We rejoice to see that he does "not, even now, consider his task as wholly completed;" but hopes to publish a "supplementary volume, in which it will further be my aim to extend and revise both the anatomical and phrenological tables, and to give basal views of at least a part of the crania delineated." We sincerely trust that the favourable reception of this volume will induce him to execute these intentions.*

On Earthquakes. By M. Edouard Biot.

M. Edouard Biot lately read an interesting memoir to the Royal Academy of Sciences at Paris, upon the ancient deluges which are mentioned in the historical annals of China, together with a catalogue of the earthquakes, and the mountainous elevations and depressions which have been observed in the Celestial Empire. Concerning this memoir, M. Boussingault, in his own name, and in that of MM. Arago and Elie de Beaumont, presented a report, which we shall now (from L'Institut, 21. Mai 1840) present to our readers.

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* *Postscript.*—On page 363, we remarked that "there is a discrepancy between the description of the ancient Puruvian skulls, and the civilization ascribed to their possessors, which is unique in Dr Morton's work." When the present sheet was in the press, we received a letter from Dr Morton, in which he says, "Since that part of my work which relates to the *ancient Peruvians* was written, I have seen several additional casts of skulls belonging to the same series, and although I am satisfied that (Fig. 4, p. 127) represents an unaltered cranium, yet, as it is the *only unaltered one* I have met with, among the remains of that ancient people, I wish to correct the statement, too hastily drawn, that it is *the cranial type* of their nation. My matured opinion is, that the ancient Peruvians were a branch of the great Toltecan family, and that the cranium had the same general characteristics in both. I am at a loss to conjecture how they *narrowed the face* in such due proportion to the head; but the fact seems indisputable. I shall use every exertion to obtain additional materials for the further illustration of this subject."

Signed, Samuel George Morton.

Philadelphia, 3d March 1840.

In the former part, the author examines the facts and traditions collected in the archives of literature, concerning two great inundations which have devastated the Chinese world. The more recent of these reaches back as far as the twenty-third century before our era, and the older is obscured in the heroic period. M. Biot undertakes to explain these catastrophes by the same phenomena of elevation, the traces of which have been pointed out by M. de Humboldt in that part of Central Asia which borders on China. The chief consideration upon which he maintains this opinion, is the coincidence which exists between the direction of the principal axis of the great American Cordillera, and the general direction of the Chinese ranges. It is this identity of direction, together with the similitude and frequency of the terrestrial commotions, which have led M. Biot to conclude, that very probably the crust of the earth is still but little consolidated, and, consequently, not very stable throughout the extent of this great circle, and that, therefore, there may have been a simultaneous elevation in the Cordilleras of the Andes and these Chinese mountain ranges which have the same direction.

This correspondence of the principal chains of the Asiatic and American continents, had not hitherto been pointed out in a way that was at all precise or satisfactory. Regarding the cause of the extreme mobility of the surface in these countries, perhaps it is to be discovered, remarks the reporter, in that volcanic zone which embraces so extended a portion of our planet, and in which we find comprehended the high mountainous ranges which constitute the most salient projections of both these continents. In truth, this zone forms an immense mountainous elevation, which runs between the Pacific ocean on the one hand, and the continents of America and Asia on the other, following, from Chili to the Burmese empire, the direction of a great semicircle of the earth. This long and uninterrupted ridge of the earth's crust, serves as a central axis to this volcanic zone, without sensibly departing from the coast line; and, as M. de Buch has already remarked, is really the natural limit of Asia; it may, in truth, be considered as separating what is now the most continental part of the globe from the portion which is the most maritime.

In the second part of his memoir, M. Biot has engaged in the collection of facts, and in proving, by human testimony, that many real modifications and changes, the dates of which are known with precision, have occurred on the surface of the globe. This is an important result, since it is thereby established, both that phenomena of a similar kind have operated in epochs which were before all tradition, and also that those catastrophes which these movements of the earth occasion, may yet again be produced. The long catalogue of events which he supplies, is made up of earthquakes, of mud eruptions, the appearance of deep gaps or clefts, of elevations occurring in the midst of plains, and of the downfall and sinking of many elevated mountains.

The catalogue of earthquakes is to be found in the 301st book of the great collection *Wen Hiom, Thong Khao*, made by *Ma Touan Lin*, a celebrated author in the 13th century of our era, whose plan was to compile together all the most esteemed works, and to classify the facts they contained, according to the Chinese notations of time. M. Ed. Biot reduces these periods to the corresponding years of our era. The catalogue of *Ma Touan Lin* had been brought down to the 17th century by Les Lettres, and M. Biot has continued it to the present time; he has likewise been so happy as throughout to add various accounts which had escaped the attention of former chronologists.

In perusing the observations of M. Biot, we are struck with the resemblance of the phenomena, and the analogy in the appearances which preceded and accompanied them; and the events of the same kind which so often occur in the Cordilleras of the New World. In Asia, as in America, after a long repose, there is seldom a solitary earthquake; there is almost invariably a succession of shocks, which follow each other at very short intervals. The first shock is only the presage of another, so that it has become a kind of proverb with the mountaineers of the Andes, that *el temblor* is always followed by *la temblora.* As to the physical accidents which result from the phenomena, we might almost say that the Chinese writers describe the convulsions of South America. In proof of this, we shall supply a few quotations.

"In the year 234 of our era there was an earthquake; immediately a great noise was heard towards the eastward. In the year 1314 there was another, which was accompanied by an explosion resembling thunder." These explosions, which it is necessary to distinguish from subterranean noises, appear to be produced in the atmosphere itself; sometimes they sound like thunder, whilst frequently they produce the sensation produced by discharges of artillery. During the great earthquake in New Grenada in 1827, M. Boussingault had the opportunity of hearing explosions of this kind. The shock occurred on the 16th of November, at six o'clock in the evening. The earth was violently agitated for about five minutes; immediately afterwards there were heard towards the south sixteen detonations, which were in every respect similar to the noise of heavy artillery. These detonations succeeded each other with a wonderful isochronism; and, although it was almost night, no light was perceived, and all the phenomena which could be collected after the event on being examined, there was no evidence that any of the neighbouring volcanoes had been the subject of an eruption: some days after, there was again heard, at eight P. M., a very loud noise, which appeared to proceed from the atmosphere. "Seventy-eight years A. C., a new peak was observed to have risen upon the *Tag-Chang* mountains. This peak was upwards of fifty feet high. In the year 780 B. C., a number of mounds of earth lengthened, and suddenly elevated themselves many feet. In the year 1599, a mountain sank down, and a lake was formed in its place; and in the middle of a flat country, five heights, great and small, appeared." It is impossible, says the reporter, at this place, not to recognise in these citations elevations analogous to those of Jorullo in Mexico, which occurred in September 1759, and of which M. de Humboldt has given a description. "In 771, a violent commotion was felt, which continued for three days;

in many places the earth was cleft, and from these openings sand issued, and stones and water, which covered all the flat country. In 1305, the earth opened up in two places, and thence issued water, which was quite black. In 1567, the earth opened in many places, and from these fissures there flowed torrents of water and black sand. In the year 125, the hill *You Tone* tumbled down and killed more than 400 persons. In 634, a mountain fell from top to bottom, and was broken into mere fragments. In 887, another mountain fell, and the sun's light was obscured by the dust. In 999, near the Yellow River, and during a time of much rain, the side of a mountain slipped down, and destroyed twenty-two families.

In regard to these latter observations, M. Boussingault makes the following remarks: "The downfall of mountains is often noticed in the Catalogue of M. Ed. Biot. These falls have not always been preceded or accompanied with oscillations of the soil. The same phenomena, and equally independent of earthquakes, frequently occurs in the Andes, especially in those regions which consist of porphyritic and trachytic rocks. Geologists have probably not sufficiently directed their attention to the fragility of the mountains of the zone in question. A number of examples might be quoted which demonstrate the identity of the phenomena. Thus, in the year 1818, at seven in the morning, a portion of the peak of Tacon, at the north-eastern extremity of the *Vega de Zupia*, slipped down, and carried along with it a hundred Indians who cultivated the sugar-cane at the foot of that porphyritic mountain. This crash produced a thick dust, which by many was mistaken for smoke. Even at the present day the debris of the rock forms a kind of slope which occupies an extent of many furlongs." M. Boussingault cites many examples of this kind, and then continues: "The geological catastrophes collected together in M. Biot's memoir appear, therefore, similar in their effects to the analogous phenomena which have occurred, and which still continue to occur, in South America; and since, in spite of the labours of the modern schools, there are still those who hesitate, and even refuse, to admit the occurrence of elevations and depressions of the earth's surface, and who, in a word, maintain the stability of the crust of the earth, it was fitting to collect, as the author has done, a considerable series of authentic facts, that they might be added to those already in our possession. The documents collected by M. Biot, by demonstrating that the still imperfectly consolidated zone of our planet is prolonged from the shore of the Pacific Ocean to the mountains of China, and that oscillations and terrestrial movements have been observed throughout this extent for nearly 200 years, will thus have contributed to supply a much more ample basis for the discussions which will yet occur concerning this portion of the physical history of the globe. Hence the members of the Commission think that they cannot too much encourage the author to pursue the course he has so happily commenced, by making his erudition and acquaintance with the oriental languages subserve the progress of science."

Results obtained by the Last Russian Expedition, sent to determine the Difference of Level between the Black Sea and the Caspian.[*]

The measurements made by Messrs G. Fuss, Sadler, and Sawitsch, were commenced on the 31st October 1836, at the town of *Kagalnik*, a little to the south of Azov, at the mouth of the Kagalnika, which falls into the Sea of Azov, in latitude 47° 4′ 26″.3 N., and longitude $2^h\,27'\,59''.5$ East from Paris. Thence they were continued by Stawropol (where the expedition passed the winter), Georgijewsk, Mosdock, and Kisljar, to the town of *Tschernoi Rynok*,[†] on the shore of the Caspian, a little to the north of the mouth of the Terek, where the operations were terminated. The whole extent of this line is about 800 versts (about 600 English miles).

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[*] From *Poggendorff's Annalen*, 1840, and it is there stated, that the information contained in the article is derived partly from the *Bulletin Scientifique de l'Academie de St Petersbourg*, vol. ii. p. 254; vol. iii. p. 27, 117, 366; vol. iv. p. 241; partly from the Dissertation by M. Alexis Sawitsch, entitled *Ueber die Höhe des Caspischen Meeres und der Hauptspitzen des Caucasischen Gebirges* (Dorpat, 1839); and partly from M. G. Sadler's Dissertation entitled, *Beobachtungen über die irdische Strahlenbrechung und über die Gesetze der Veränderung derselben* (Dorpat, 1839.)—Edit.

[†] In the country around Tschernoi Rynok, the observers found a general conviction among the inhabitants of the shores of the Caspian, that that sea was gradually receding. The older peasants of this fishing town know, that, thirty years ago, the water reached quite close to the town, while at present

The measurements were of two kinds,—Trigonometrical and Barometrical; and the following are the results obtained:—

Height of the Sea of Azov above the Caspian.

Calculated by M. G. Fuss (*Bull. Scient. de l'Acad. de St Petersb.*, 1838, vol. iv. p. 241.)

According to the method of simultaneous, reciprocal, Zenith-distances, ... 73.1 feet English.

According to the method of the mean of the Zenith-distances, ... 75.2 ...

Calculated by M. Sawitsch (in his Dissertation, 1839, p. 22.)

According to the method of simultaneous, reciprocal, Zenith-distances, ... 78.1 feet English.

(Probable error = 3.5 feet.)

According to the method of the mean of the Zenith-distances, . . . 82.5 ...

(Probable error = 5.2 feet.)

Calculated by M. Sadler (in his Dissertation, 1839, p. 33.)

According to the method of simultaneous, reciprocal Zenith-distances, ... 83.3 feet English.

(Probable error = 2.4 feet.)

According to the method of the mean of the Zenith-distances, . . . 81.3 ...

(Probable error = 3.9 feet.)

The discrepancies in these results arise from the way in which the observers have calculated the measurements for the purpose of removing the effect of terrestrial refraction. We cannot decide as to which of these results is most probably the correct one; but it is evident that, as the uncertainty amounts only to a few feet, we shall not be very far wrong in assuming, as the number most nearly approaching the truth, the mean of the four last (which are those that agree the best with one another), viz., 81.3 English feet. Even should this number

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there is an intervening piece of extremely flat ground, three or four versts in breadth (two or three miles English). When, likewise, they reached the point forming the water-shed of the two seas, they were struck by the different aspect of the surface of the country presented to them. The tract is doubtless the former bottom of the Caspian.

not be regarded as a definitive result, yet, at all events, by means of this first trigonometrical survey of the Caucasian Isthmus, the fact is established, *that the surface of the Caspian Sea is actually at a lower level than that of the Ocean*, an opinion which has been lately disputed; and it is likewise ascertained, *that the depression is only about one quarter so great as the older measurements led us to believe.*

Respecting the Barometrical measurement, or the second portion of the operations of this expedition, M. Sawitsch has communicated the following information to Baron A. von Humboldt:—"Besides the result of the Trigonometrical survey, we are also in possession of two Barometrical results. The first was obtained by means of observations made at all the intermediate stations,—stations which were nearly a German mile apart (about 4⅔ English miles). Our instruments were cistern-barometers, like those employed by Parrot. They, as well as the thermometers, were compared daily. At each station, three observations at least were made between half-past three and half-past six P.M. The difference of level between two adjoining stations generally afforded a satisfactory approximation; but, during high winds, errors occurred, which for some time were all on one side. These errors of the partial measurement were particularly great when a violent gale prevailed, or when there was a regular storm. The definitive result is far removed from the truth, inasmuch as it assigns as the difference of level of the two seas, *three hundred* English feet, instead of only *eighty*, which it, in fact, amounts to. The discrepancy between the two results is too great to admit of its being ascribed solely to accidental errors of observations. The sum of these errors cannot amount to more than 30 or 40 feet. It may, probably, be rather attributed to atmospherical influences, which are not always of the nature assumed in the usual formulæ for the calculation of observations. The second barometrical result is founded on a year's observations made with compared instruments at *Taganrog* and *Astrachan*. These give, as the difference of level, nearly *one hundred and forty English feet*, a result which comes nearer to the truth than the first. At present, an excellent series of observations is in the course of being made at *Astrachan* and *Nicolajeff*, which promises to afford a valuable result."

Notice of Professor Schouw's Work on the Climate and Vegetation of Italy.

THE present happy state of development of the physical sciences, allows us to treat collectively of different portions of physical geography which were formerly separated. The floras of our time are more than mere pieces of nomenclature; and the meteorological observations are more than mere figures, without meaning.

The labours of Humboldt, Buch, and Wahlenberg, which have chiefly contributed to this union of the physical sciences, excited an early interest on the part of the author in the geographical part of phytology. A journey to the mountains of Norway, undertaken in 1812 in company with Christian Smith, who afterwards fell a victim to his zeal for the sciences at Congo, further contributed to decide the direction of his studies. Humboldt had presented a picture of the great physical relations in the tropical regions of the New World,—Von Buch and Wahlenberg had described Scandinavia, the Carpathians, and the northern portion of Switzerland; and it therefore appeared to the author that an account of the physical relations of an intermediate region could not fail to be of great interest; and no country seemed more suited to this object than Italy, from its presenting so much variety, owing to its considerable extent from north to south, and from its possessing the Alps as its frontier, being traversed by the Apennines, including various volcanic tracts, and exhibiting such a development of coast. Moreover, there is no other country in the same latitude where plants have been so much examined, and where so many meteorological observations have been made, investigations essential for the exact determination of general physical relations. So early as the year 1815, the author sketched out a tour in Italy with this object, a journey which met with the approbation of his master and paternal friend Mr Hornemann; and, by the advice of his scientific counsellors, the King of Denmark, always zealous in the cause of science, was pleased to present the author with the means of realizing his projects.

Having prepared himself for his undertaking at home and in Germany, the author arrived at Trieste in the spring of 1817, and employed the summer in exploring the southern portion of the Alps between Carniola and Mont Cenis. In the autumn he crossed the Apennines to Genoa, traversed various portions of the Ligurian mountains, and passed the winter at Pisa and Florence. In 1818, he penetrated the Apennines, ascended their highest summits in the Abruzzi and in the States of the Church, and followed the whole chain to the most southern part of Calabria. Thence he crossed to Sicily, ascended Etna, traversed the interior of Sicily, and visited the Nebrodic Mountains. He passed the winter of 1818–19 at Rome and Naples; and in the succeeding spring he returned to Sicily, occupied himself with the southern and eastern coasts, and again ascended Etna, where he was the spectator of a considerable eruption; after which, he took a general view of the mountains of the north-eastern portion of the island. While traversing Italy on his way homewards, he had again an opportunity of examining some parts of the Apennines and of the Alps. He crossed the latter in the autumn of 1819; and, after passing some time at Geneva and Paris, he returned to his native country in the beginning of 1820.

The author commenced the preparation of his materials; but his labours advanced but slowly, and were interrupted by his various other studies. The latter, and particularly his treatise on the Universal Geography of Plants, and his description of the climate of Denmark, led him to investigations intimately connected with those which necessarily formed the object of his work on the climate and vegetation of Italy. During his studies, he felt more and more the necessity of again visiting Italy; and the munificence of the king enabled him to undertake his second journey, which he commenced in 1829.

The first summer was chiefly employed in visiting a portion of the Apennines of Tuscany and Modena, and the following spring in exploring the coasts of Sicily and the environs of Naples: the next summer was occupied with researches in the mountains of Sora, the Apuanian mountains, those of

Carnia, and many other portions of the Apennines and Alps, which he had no opportunity of seeing during his first journey. His collection of meteorological observations was considerably augmented by his second journey.

It is the results of these different journeys which the author proposes to include in a work on the climate and vegetation of Italy, of which the first volume is at present published. But it is only by comparing different countries that the climate and vegetation can appear in their true light: it is the comparative method which has rendered the physical researches of our times so superior to those of former periods, both in precision and in compass. The climate and the vegetation of Italy will not be discussed in an insulated manner, but along with the climate of the rest of Europe, and of Northern Africa. The work will be composed of two chief parts, the one devoted to the climate, and the other to the vegetation, each forming an independent treatise. The present volume contains an account of the temperature and the rain; and, as the configuration of the surface exercises an essential influence on the climate, it has been premised by an orographical summary; although it must be mentioned that this summary, and the illustrative map, are not so complete as they ought to be for the special orography of Italy. The second volume will contain the other elements of climate, and a comparison of the different years relatively to their meteorological character, which will lead me to treat of the interesting question of the variations attributed to climate. The third volume will be entirely dedicated to Phyto-geography.

Several years have elapsed since the author's second journey; but the duties of his office, and occupations of quite a different description, have retarded the publication of the work. The observations collected in Italy were, however, calculated several years ago, and partly printed in the supplement. Thus the author has been very rarely able to take advantage of observations and writings of a more recent date.

The author was perfectly aware of the great difficulties to be encountered by a traveller in giving an account of the physical state of a country, and of the imperfections which necessarily result. Much local and detailed information, within the reach of an inhabitant of a country, escapes a traveller, especially when he is exploring a very large country, and cannot remain a sufficient time at each station. But the author hopes for an equitable and indulgent judgment; and he flatters himself that the scientific men of Italy will receive the work with the same kindness that they shewed to the author himself. A great part of the work belongs, of right, to Italy, for it could not have been written without the numerous observations and researches of Italian *savans*, and without their kindness.*

On the Climate of the portions of Europe and Africa which are situated between the Equator and 60° North Latitude.

By Professor SCHOUW.

1. *Thermographical Conclusions.*

1. At the level of the sea the mean temperature of the year in Europe and Africa, between the equator and 60° N., varies from 81°.5 to 32° F.

2. The isothermal lines are the most separated (that is the temperature diminishes the most slowly) between the equator and latitude 25° N.; between 30° and 45° they approach one another more; and in a higher latitude the intervals again become more considerable.

3. In advancing from west to east in the above-mentioned part of the globe, the isothermal lines have equatorial inflexions but these inflexions become more considerable as the Polar Circle is approached. The most northern point (the polar summit) in the curve of the isothermal lines between 32° and 59° F. is on the coasts of the Atlantic; and this point in the lines of more elevated temperatures is situated more to the east (on the shore of the Mediterranean between Spain and Italy, and in the interior of Africa).

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* This notice forms the introduction to the 1st volume of this valuable and important publication. The title is: *Tableau du Climat et de la Vegetation de l'Italie, par J. F. Schouw, Professeur de Botanique à l'Université de Copenhague. Copenhague*, 1839.—EDITOR.

4. The difference of the temperatures of the seasons, and of the months, augments not only from the equator towards the Polar Circle; but, in the portion of the globe under consideration, it likewise increases from west to east, *i. e.* from the coast of the Atlantic towards the interior of the continents.

5. Between 60° and the Tropic of Cancer, the hottest months of the year occur at a later period in proportion as the geographical position is more northern.

6. In the same latitude the hottest months occur at a later period in proportion as the sea is approached. Thus in comparing the months of June and September we have,

	Sept.=June.		Sept.=June.		Sept.=June.
Ullensvang,	—3°.96 F.	Stockholm,	—5°.22 F.	St Petersb.	—7°.92F.
Edinburgh,	—1°.26	Copenhagen,	—3°. 6	Moscow, .	—19°.98
Paris, . .	—2°. 7	Carlsruhe,	—4°.32	Ofen, . .	—5°.58
Bordeaux,	+0°.18	Nice, . .	+1°.44	Florence, .	—1°.62
Lisbon, .	0 . 0	Palermo, .	+1°.62	Canea, . .	—1°.98
St Cruz, .	+3°.42	Tunis, .	+3°.06	Cairo, . .	—0°.54

Hyetographical Conclusions,

The *zone of summer rain* extends in Africa from the equator to the 15th degree N. latitude. The quantity of pluvial water is very considerable, but the rains do not fall except in the months of northern declination of the sun.

The *zone without rain* includes Northern Africa, between 15° and 30° N. It is the zone of deserts, and is either entirely devoid of rain, or the rains are very rare, and entirely accidental.

The *zone of winter rain* includes Northern Africa and Southern Europe, between 30° and 45° N. latitude. The rains are almost confined to the months of low temperature (in the southern portion of the zone), or they are very frequent in these months, and rare in those of an elevated temperature. The quantity of pluvial water augments towards the Atlantic, but still more towards the mountains; above all, it is very considerable on their northern and western slopes, but very small on the *plateau* of the Spanish peninsula.

The *zone of continued rain* includes the portion of Europe between 45° and 60° N. The aqueous precipitations are frequent in all seasons, and the difference in quantity between these is not considerable. In the islands and on the western shores the rain is most abundant in autumn, and pretty abundant in winter; in the interior of the continent it is the summer which presents the greatest amount of rain. The coasts of the Baltic exhibit intermediate relations in this respect. The annual quantity of rain depends more especially on proximity to mountains. In the great northern plain of Europe, while the annual quantity augments towards the Atlantic, it only presents slight differences; but its increase becomes very considerable as we advance from the plains towards the secondary and less elevated masses of mountains (for example, the *Vosges* and the Sudetic mountains), and it is still greater towards the great systems (the Alps and the Scandinavian system). The largest quantities known are those of some places on the northern slope of the Alps and the Apennines; and on the western slope of the Scandinavian system, and of the mountains of the British Islands.*

Researches on the Vital Heat of Cold-blooded Animals.

By M. DUTROCHET.

Animals may be divided into two great sections in reference to the degree of their vital heat. 1. Those of a high temperature—the so-called warm-blooded,—a section containing birds and mammifera; 2dly, those of a low temperature,—the cold-blooded,—a section including reptiles, fishes, and all invertebrate animals. The vital heat of animals of a high temperature can be easily determined, because it is usually much above the temperature of the surrounding medium. It is otherwise with animals of a low temperature, and is very difficult to determine, on account of its slight elevation above the surrounding medium, the temperature which the animal acquires, and to which it adds that which is peculiar to itself. I shall not here consider all the difficulties which must be overcome, in order to distinguish accurately the faint heat peculiar to the animal among the anomalies produced by the heat continually communicated to it or excited in it, by the surrounding medium, and by the heat which it is always receiving or losing by radiation. All these matters shall be explained in my Memoir, as well as the reasons which have led me to think, that the thermo-electrical apparatus is much preferable to the thermometer, for determining the vital heat of animals with a low temperature. This apparatus I have accordingly used, and I have followed the same plans of experiment which I have described in detail in my researches on the vital heat of vegetables. I have taken care, likewise, to prevent the cold caused by evaporation, by placing the animal under observation in air saturated with water. Without this precaution, I could not have obtained the whole vital heat of the animal, which, in many circumstances, would have been found to be even colder than the surrounding air. I now proceed to give a very brief notice of the results of my observation.

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* These general conclusions are given by Professor Schouw in the Atlas prepared for his work on Italy by Captain O. N. Olsen.—EDITOR.

Reptiles.—Different observers have estimated the proper heat of the frog (*Rana esculenta*) Linn., from 0°.59 F. to 4°.5 F., (from ⅓ of a degree to 2½ degrees centesimal) above the temperature of the surrounding medium. Berthold alone found that this reptile, when observed in the air, was colder than that medium, and when in water, it had the same temperature as the liquid. He found no exception to this but when the frogs were copulating, when their internal heat was raised, 1°.8 F. (a centesimal degree) above the temperature of the water in which they were living. I have had no opportunity of observing the vital heat of frogs during copulation; I have experimented on them only in a single state, and not in the season of their copulation. I thrust one of the needles of the thermometrical apparatus, sometimes into the abdomen, at other times into the muscles of the thigh. In this manner I have ascertained, that, in the open air, the frog is about 1°8. F. (1°.0 Cent.) colder than the surrounding air; and that, when placed in air saturated with water, it indicates a vital heat of from 0.054, to 0.09 of a degree of Fahrenheit (from 0.03 to 005 of a Cent. degree) above the temperature of the ambient medium. I obtained the same result from experiments on a frog plunged in water. The relative cold of the animal placed in the open air arises, therefore, from the coldness produced on its surface by evaporation.

The tadpoles of the frog presented no appreciable degree of vital heat.

Bufo obstetricans, observed in the open air, was found colder than that medium by 1°.35 F ($\frac{3}{4}$ of a cent. degree); in air saturated with moisture, its proper heat was 0°.22 of a degree of F. (0°.12 Cent.). Its tadpole offered no appreciable vital heat.

The vital heat of the grey lizard (*Lacerta agilis*, Linn.) has been estimated by Czermak to be, from 2°.25 F. to 14°.61 F. (from 1°.25 C. to 8°.12 C.) The striking difference in the results obtained, leads us to suspect that there must have been some error. In my own experiments, I have found that this same lizard, in the open air, was colder than the surrounding medium from 0°.32 F. to 0°.305 F. (0°.18 C. to 0°.20 C.). In air saturated with water, the vital heat appeared to be 0°.38 of a degree of Fahrenheit (0°.21 of a cent. degree).

It will be remarked, in these observations, that the feebler the vital heat is in reptiles, the more decidedly they are aquatic. The lizard, which has the greatest degree of heat of the three reptiles made the subjects of observation, inhabits dry places; the toad frequents holes in moist earth, and its vital heat, inferior to that of the grey lizard, is higher than that of the frog which lives in water. Finally, the tadpoles, which live exclusively in water, have such a low degree of vital heat, that it cannot be appreciated by any thermoscopic means.

Fishes.—I describe in my memoir the researches made by various observers on the vital heat of fishes; and shall refrain from introducing them again in this extract. The result of these researches assigns pretty generally to fishes, a vital heat somewhat higher than the water in which they live. Certain observers, however, of high repute, have found their temperature to be similar to that of the surrounding water. Such was the conclusion arrived at by MM. Humboldt and Provençal, by MM. Prévost and Dumas, and by M. Berthold. The only fish which I have subjected to experiment, is the *Cyprinus alburnus*, and I have always found it, when in water, preserve the same temperature as that liquid, manifesting no appreciable vital heat.

Mollusca and Annelides.—Berthold has shewn by experiment that the *Helix pomatia* and slugs have a temperature inferior to that of the surrounding air, and that these mollusca, when plunged in water, have the same temperature as the liquid. He obtained the same results with *Anodonta anatina*, with the medicinal leech, and earth-worms. My own experiments have demonstrated that the *Limax rufus*, *Helix pomatia*, and *Hirudo medicinalis*, when observed in the air, are colder than that medium, and that when placed in air saturated with water they manifest no proper heat.

Crustacea.—All observers, with the exception of M. Valentin, agree in attributing no vital heat to crustaceous animals. All these crustacea possess only the temperature of the water in which they live. I have been led to the same result by experiments made on the craw-fish (*Astacus fluviatilis*). Neither in moist air nor in water have I observed in it any vital heat. The needle of the instrument was inserted into the abdomen. It is well known that the crustacea breathe by bronchiæ like fishes; the absence of appreciable vital heat in both is probably owing to this mode of respiration.

Insects.—The most extensive researches that have been made on the vital heat of insects are those published by Mr Newport in the Philosophical Transactions for 1837. This observer employed small thermometers, one of which, usually inclosed in a glass-phial, was applied to the body of the insect, while another themometer, not inclosed, indicated the temperature of the surrounding air. Once only he introduced the ball of the thermometer into the body of the cockchaffer. My own researches on the subject have been made by inserting the needle of the thermo-electrical apparatus into the abdomen of the insect. I have in this manner obtained the exact degree of its internal heat. Nobili and Melloni had formerly applied this instrument to ascertain the vital heat of insects, but only by measuring the radiated heat that emanated from their bodies. They have neither given the names of the insects subjected to observation, nor any measurement of their heat: they are satisfied with saying, " It may be admitted as an indisputable fact, that insects have a temperature, although little higher than that of the surrounding medium." *

According to Mr Newport, the vital heat of insects is greater in the perfect state than in the larvæ; higher in a state of excitement than of repose; higher when the insect is awake than when it sleeps; it is diminished by want of food, and increases with the frequency of the pulsations of the dorsal vessel, and with the activity of respiration. Mr Newport thinks that, by augmenting at pleasure the activity of respiration, insects can by that means voluntarily increase the degree of their vital heat.

I have been unable to repeat all Mr Newport's experiments. The mode of experimentation employed by me necessarily threw the insects I was observing into a state of excitement; they were either fixed immoveably to the spot, or in a state of agitation, although retained in a particular place. I refer to my memoir for an explanation of my method of proceeding; I limit myself here to a comparison of the results at which I arrive with those of Mr Newport. I have reduced the degree of Fahrenheit's thermometer, which the English observer made use of, to centesimal degrees: I cannot notice all his experiments, for they are very numerous.

Mr Newport found in the *Bombus terrestris* a vital heat of from 0°.99 F. to 9°.36 F. (0°.55 to 5°.2 C.), according as it was in a state of rest or excitement. In my experiments, the heat of this insect was not raised above 0.45 of a degree of F. (a quarter of a centesimal degree). I found the same vital heat in *Bombus lapidarius*, *Bombus hortorum*, and *Xylocopa violacea*.

The larva of the common cockchaffer (*Melolontha valoris*) afforded to Mr Newport a heat of 0.59 of a degree of Fahrenheit, or 0.33 of a centesimal degree; in my experiments it did not exceed 0.04 of a centesimal degree, equal to 0°.07 F. The cockchaffer, in its perfect state, having the ball of the thermometer sunk in its abdomen, was found by Mr Newport to have a vital heat of 2°.79 F. to 4°.68 F. (1°.55 to 2°.6 Cent.). I never saw it exceed 0.32 of a degree of F., or 0.18 of a centesimal degree, the insect being in an immoveable state. When in a state of agitation, Mr Newport found that the cockchaffer had a heat of 9° F. (5° Cent.) above that of the surrounding air;

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* *Annales de Physique et de Chimie*, t. 48, p. 207.

while, in similar circumstances, I found it not to surpass a quarter of a Centesimal degree. *Melolontha solstitialis* possesses nearly the same vital heat as *M. vulgaris.*

Mr Newport studied the vital heat of *Lucanus cervus* by placing the ball of a thermometer under its elytra; he found it to be about 1°.58 of F. (0.88 of a degree Cent.) when the insect was in repose, and 2°.52 F. (1°.4 C.) when agitated. In my experiments on the same insect, fixed in an immoveable posture, there appeared a proper heat of 0.39 of a degree of F. (0.22 of a degree Cent.), and this rose to 0.9 of a degree F. (half a degree Cent.) in another example of the insect in a state of the most violent agitation.

In all these experiments, the insects I observed were placed in air saturated with water; when I placed them in the open air, their internal heat became scarcely superior to that of the surrounding atmosphere, and was even at times below it.

The following are the degrees of vital heat I have ascertained to exist in certain other Coleoptera, placed in saturated air.

Carabus auratus, Fab.	0.32 F. (0.18 C.)	of a degree.
... mobilis, do.	0.32 F. (0.18 C.)	do.
Blaps mortisaga, do.	0.21 F. (0.12 C.)	do.
Cetonia aurata, do.	0.45 F. (0.25 C.)	do.
Chrysomela tenebricosa, do.	0.61 F. (0.34 C.)	do.
Scavabacus vernalis, do.	0.38 F. (0.21 C.)	do.

According to Mr Newport, the vital heat of *Gryllus viridissimus* rises to 4°.68 F. (2°.66 C.); in my experiments on the same insect I did not find it above 0.61 F. of a degree (0.34 of a degree Cent.) In the open air this insect is colder than the surrounding medium.

Gryllus verrucivorous, Linn., and *G. campestris*, appear, by my observations, to have a vital heat of about 0.9 F. (0.4 R.) of a degree. That of *Gryllus grillotalpa*, Linn., is less, not exceeding 0.28 F., or 0.16 of a degree Cent.

The following are the degrees of vital heat which I have found in certain Lepidoptera, respectively in the larva, nymph, and perfect insect.

Sphynx Stellarum, Linn. larva, .	0.198 F. (0.11 C.)	of a degree.
Id. perfect insect, . .	0.52 F. (0.29 C.)	do.
Sphynx Tiliæ, larva about to undergo its metamorphosis, . .	0.77 F. (0.43 C.)	do.
Id. nymph a month old, .	0.41 F. (0.34 C.)	do.

Sphynx Atropos, 24 hours after assuming the perfect form, ...	1°.04 F. (0.58 C.)

The latter is the highest degree of vital heat which I have observed among insects.

These observations shew, that the vital heat of animals, with a low temperature, is generally very much inferior to what had been previously assigned them, for it is found that, at its maximum, it does not reach 1°.08 of Fahrenheit.

On taking a general glance at living beings with a low temperature, whether vegetable or animal, it is seen that their vital heat is in relation with the activity of their respiration, and also with the physical condition of the respired air. It is almost always appreciable in animals which respire elastic air; there is no exception to this heat in the gasteropodous mollusca, whose lungs are very small, and seldom renew the air which they contain. With regard to animals which respire air under water by means of gills, their vital heat is so low, that it does not manifest itself, even by employing the most delicate thermo-electrical instruments. It is allowable to suppose that this want of appreciable vital heat, is owing to oxygen in the water, when it becomes fixed in the act of branchial respiration, giving out only a small quantity of caloric comparatively to what it yields when fixed by the act of pulmonary respiration, or by that of tracheal respiration which is peculiar to insects.

Vegetables likewise respire elastic air, by respiratory organs, in a state of high development, and, moreover, it is not atmospheric air which they introduce into their organs, but the oxygen gas disengaged by their green parts under the influence of light. Their vital heat, therefore, ought to be at least equal and sometimes higher than that of certain insects or reptiles. This is what I have observed, and not without surprise. Is it not, in fact, surprising that a plant, *Euphorbia lathyris*, for example, should have a vital heat, which, at its maximum, is ten times greater than that of a frog, and infinitely greater than those of fishes, and all other animals that breathe by gills? The family of Aroïdes, among plants, has a vital heat in the spadix of their flowers, of so high a temperature, that it surpasses what has been observed among animals, excepting those with warm blood, insomuch that, in this point of view, vegetables exist which hold the first rank among living beings with a low temperature.

Why do living beings present only two conditions of existence in regard to the degree of their vital heat? Why are some of a high temperature, while in others it is very low, without any existing which, in their normal state, possess a temperature intermediate between these two extremes? I am aware that warm-blooded animals, in a state of hybernation, have an inferior vital heat to that which attends their normal condition, and higher than that of cold-blooded animals; but that does not constitute a normal condition of existence intermediate to that of warm-blooded animals in their normal state, and that of animals with cold blood. The warm-blooded animal, whose vital heat is diminished while in a state of hybernation, enjoys life but imperfectly, and would soon perish if that state were prolonged. It may therefore be established, as a general law of nature, that the vital heat of living beings, whether vegetable or animal, must either be so faint, that it is often impossible to recognise its presence, or so elevated as to approach that degree in which the existence of life, and especially of animal life, becomes impossible. The degree of constant heat, which is incompatible with the normal and permanent existence of animal life, appears to be about 122° F. (50°. Cent.); for the vital heat of birds is so high as 111° F. (44° Cent.) *To possess an extreme degree of vital heat, or almost none*, appears, therefore, to be the law to which all living beings are subjected. The existence of this law is inferred wholly from the fact of its generality; for we perceive no reason for its necessity. *Living creatures with a low temperature*, in order to live in their normal condition, must necessarily derive heat from the surrounding medium; *living beings with a high temperature*, on the contrary, in order to live in their normal state, must necessarily lose heat by imparting a portion of what they produce to the medium which surrounds them. The former must, therefore, be placed in a medium warmer than themselves, and the latter in one colder; for no animal with a high temperature or warm blood can live in a medium equal in warmth to its own, and much less in one that is always superior. The injurious effect which it would experience from the elevated temperature, would be greater in proportion to the density of the surrounding medium. It has been ascertained by observation, that animals of a low temperature, can endure, in certain cases, a constant ambient heat much greater than that which warm-blooded animals could resist for any great length of time. Thus, certain fishes living in thermal springs, the temperature of which rises to 104° F. (40° C). Even a higher temperature is assigned to some of the springs in which fishes live, but we may be permitted to presume that there is some error in the observation.

Extraordinarg Land-Slip and great Convulsion of the Coast of Culverhole Point, near Axmouth. By the Rev. W. D. Conybeare.

The recent season of Christmas has been marked on the neighbouring line of coast by a convulsion so remarkable, from the extent, magnitude, and picturesque changes it has produced in the surface and general configuration of a line of country, extending at least a mile in length, by half a mile in breadth, (including the farms of Dowlands and Great and Little Bendon), that I conceive some account of it cannot fail to be acceptable. Although this convulsion can only be ascribed to the less dignified agency of the land-springs constantly undermining the substrata; yet in the grandeur of the disturbances it has occasioned, it far exceeds the ravages of the earthquakes of Calabria, and almost rivals the vast volcanic fissures of the Val del Bove on the flanks of Ætna.

Convulsions of a similar nature have in former centuries impressed on the whole line of coast between Lyme and Axmouth, a character of wild and romantic picturesque combinations of scenery scarcely rivalled in any other portion of the British coast. The Undercliff in the south of the Isle of Wight, affords another example of the similar effects resulting from the agency of the same causes on rocks of the same geological formations, and having a like position in relation to the line of coast; it may, however, be confidently asserted, that the Undercliff of South-eastern Devon, in the scale and picturesque effect of its features, is far superior to that of the Isle of Wight.

It will be necessary, in order to give a clear idea of the causes which have produced these convulsions, to premise a few words as to the nature and distribution of the rock masses on which they have acted. The tract of Downs ranging along the coast is here capped by a stratum of chalk, this rests on series of beds of consolidated sandstone, alternating with seams of that variety of flints called Chert; beneath these more than one hundred feet of loose sand, locally (from an obvious etymology) termed fox mould; this bed affords the principal cause of the disturbances in question, for it imbibes all the atmospherical water falling on the surface, and as it rests on retentive beds of clay (belonging to the lias formation) these waters are here held up and flow out in springs along the margin of this deposit wherever it is exposed by the slope of the ground, as is necessarily the case all along the face of these Downs where they break down to the sea; the springs thus issuing wash out with them a very sensible portion of the loose deposit of fox mould, through which they flow—and such an action is of course greatly aggravated by the inordinate continuance of wet weather in a season such as has lately prevailed. Thus considerable portions of the fox mould being gradually removed along the lines through which these subterranean springs have found their course, the superstrata will remain completely undermined; and as an excessively wet season will, by saturating the whole with moisture, increase the weight of the incumbent mass, at the same time that (as we have seen) it withdraws the support, it is easy to conceive that cracks will in process of time be formed, and that the undermined portions of the superstrata will be precipitated into the hollows prepared beneath them. And further, as the adjacent masses of rock, even where not thus completely undermined, rest on a slippery basis of watery sand, the motion originally impressed by the falling in of an actually undermined tract will readily be propagated to a considerable extent in an internal direction.

These causes having acted through centuries, have produced a series of dislocations affecting all the seaward face of the range of hills lining this part of the coast for an interval of more than a furlong, from the sea-beach inland. The whole of this interval presents the wildest scene of ruin imaginable, —"crags, knolls, and mounds, confusedly hurled" in a succession of broken terraces, separated by deep and thickly wooded dingles, an inland range of chalk cliffs mantled by luxuriant screens of ash and elm, wherever the declivity will allow a root to fix itself, forming the upper stage and general background of the scene, and extending to the very summit of the hills. This general character prevails through the undercliffs of Pinhay, Whitlands, Rowsedown, Dowlands, and Bendon, the latter of which has been the principal scene of the convulsion, which in the last week has added new features of such magnitude and interest to those which previously marked this range of coast. I proceed to my narrative. On the morning of Tuesday the 24th, at about three o'clock A.M., the family of Mr Chapple, who occupied the farm of Dowlands, about half a mile from the commencement of the disturbances which ensued, was alarmed by a violent crashing noise; but nothing farther was observed through that day. On the following night, however, about the same hour, some labourers of Mr Chapple, the tenants of cottages built among the ruins of the adjoining undercliff, hurried to the farm, with the information that fissures were opening in the ground around, and the walls of their tenements rending and sinking. Through the course of the following day (Christmas) a great subsidence took place through the fields ranging above Bendon Undercliff, forming a deep chasm or rather ravine, extending nearly three quarters of a mile in length, with a depth of from 100 to 150 feet, and a breadth exceeding 80 yards. Between this and the former face of the undercliff extends a long strip, exhibiting fragments of turnip-fields, and separated from the tract to which they once belonged by the deep intervening gulf, of which the bottom is constituted by fragments of the original surface, thrown together in the wildest confusion of inclined terraces and columnar masses, intersected by deep fissures, so as to render the ground nearly impassable. The insulated strip of fields also which has been mentioned, is greatly rent and shattered. The whole of the tract which has been subjected to these violent disturbances, must be estimated, on the most moderate computation, as exceeding three quarters of a mile in length by 400 feet. The extent of injury sustained by the adjoining farms, may be readily estimated from these data—which, from the fear of exaggeration, I am, however, persuaded I have greatly understated. The whole of the adjoining undercliff between this new fissure and the sea has been greatly affected by the lateral movements to which I have before alluded: the whole surface is corrugated by new ridges and furrows, and traversed at every step by new fissures; and the whole line of sea-cliff has completely changed the features it possessed a week ago, and been very generally moved bodily forward for many yards. A remarkable pyramidal crag off Culverhole Point, which lately formed a distinguished landmark, has sunk from a height of nearly 100 to 20 feet, and the main cliff, before more than 50 feet distant from this insulated crag, is now brought almost close. This motion of the sea-cliff has produced a further effect, which may rank among the most striking phenomena of this catastrophe. The lateral pressure thus occasioned, has urged the neighbouring strata extending beneath the shingle of the shore by their state of unnatural condensation, to burst upwards in a line parallel to the coast; thus an elevated ridge, more than a mile in length, and rising more than forty feet, covered by a confused assemblage of broken strata, and immense blocks of rock, invested with sea-weed and corallines, and scattered over with shells and star-fish, and other productions of the deep, forms an extended reef in front of the present range of cliffs: this terminates at its eastern and western extremity in two deep basins of water. The western of these basins is encircled by the extreme arm of the new reef, in such a manner as nearly to resemble the Cobb at Lyme; which, however, it exceeds in size.

The singularity and picturesque effect of the new combinations produced by this remarkable convulsion, must be sufficiently evident from the above description, without swelling a communication already too long, by any vain attempt to delineate by the pen that which were a fitter subject for the pencil.*

Axminster, *Dec.* 31. 1839.

On the Lines of the Ancient Level of the Sea in Finmark.

By M. Bravais.

In a memoir communicated to the French Academy of Sciences, M. Bravais has brought together the results of measurements of height made of the lines which denote the former level of the sea in Finmark in Norway, between the 70th and 71st degrees of northern latitude. These observations were carried on during a residence of a year in that district, and during the leisure left him while fulfilling the special mission assigned him of prosecuting meteorological and astronomical researches; and his other occupations prevented him from collecting a more complete amount of information on this question.

The field of his researches embraced an extent of about 18 marine leagues, from the small town of Hammerfest to the mines of Kaafiord, at the inner extremity of the bay of Alten.

The author distinguishes two lines of ancient level extremely well marked. The upper line has an elevation of 67.4 metres † in the Kaafiord, and its elevation diminishes gradually as far as the mouth of a river called the Jern-elv, where it is not more than 42.6 metres. From that point it diminishes in a much more rapid manner as far as Hammerfest, where the height is only 28.6 metres. The lower line follows similar phases, but its inclination is regular, and about 35″ of a degree; its altitude near Bossekop in the Altenfiord is 27.7

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* We are glad to remark the announcement of a series of plates illustrative of the above described subsidence of land, and which is to be accompanied by a geological memoir, descriptive of this and similar phenomena, from the pen of the Rev. W. D. Conybeare, the whole to be revised by Dr Buckland. We have seen two of these plates, viz. "A View of the Axmouth Landslip from Dowlands," and "A View of the Landslip from Grand Bindon;" both of which are from drawings by Mrs Buckland, and are not only remarkable for their beauty of effect, and for the skill in delineation they exhibit, but likewise bear marks of minute geological accuracy.—Editor.

† A metre, equal to 3 feet 3.37 inches English.

metres, and at Hammerfest it is only 14.1 metres. Thus these lines are neither horizontal nor even parallel with each other.

There is also a third line which is less evident, and whose reality may be disputed; it has a height of 40.5 metres in the bay of Alten, but only 21 metres near Hammerfest.

The marks by which these lines are recognised are the following: 1*st*, At the mouths of important valleys, horizontal plateaus, formed of heaped-up loose matter, and termed *terraces;* these are the ancient deltas of the river-courses which flowed in the valleys. 2*d*, Lines of *erosion* on the rocks; these are spaces sensibly horizontal to the eye, where the rocks are corroded and full of holes for a height of one or two metres; seen from a little distance these lines appear like great black streaks. 3*d*, Lines which the author terms lines of *redressement* or of *resaut* (projection), on account of the movement which elevates the surface above the line. The line itself generally forms a sort of band more or less even, which winds horizontally along the declivity of the mountains, and which resembles the bank of a canal or the *banquette* of a fortification.

These three kinds may be substituted for one another, or may be placed in juxtaposition, according to the variation of local circumstances; thus, to a terrace may succeed a line of erosion, or of projection, or conversely. The coincidence of the extreme portions of the two modes of formation can be recognised at a simple glance, or at least by ascertaining that the level has remained the same. There are also long intervals in which no vestiges of these lines are to be met with; but they reappear a little further on, in such a manner as to leave no reasonable doubt of their identity with those previously observed.

After some remarks on the circumstances which may determine these interruptions, and the marine shells met with in these raised sea-beaches, the author examines rapidly the principal hypotheses which can afford a key to these phenomena, and is of opinion that that of *soulèvements* is the most probable. A list follows of the facts of the same kind noticed by other observers on the coast of the kingdom of Norway; and the memoir is terminated by a table of hypsometrical measurements, a map of the locality, and a plate of sections.

From all the facts enumerated it would seem to result, in accordance with the opinion of Professor Keilhau, that Norway has been elevated by jerks *(par saccades)*, and that the entire change is the sum of a certain number of successive changes, which have alternated with long periods of perfect repose. The same phenomenon must have been common in Sweden, at least in the southern portion of that country.—(Comptes Rendus, 1840, 1er Sem., p. 691.)

Death of Olbers.

Astronomy has sustained a great loss by the death of the learned and venerable Dr Olbers, which took place at Bremen on the 2d of March 1840, in the 82d year of his age. Until sufficiently full biographical notices give an account of his life and labours, we think it our duty to pay a short tribute to his memory. William Olbers was born on the 11th October 1758, at Arbergen, near Bremen, where his father was clergyman. He took the degree of doctor at Göttingen, on the 28th December 1780, after having previously made himself known as an astronomer; and, while he practised medicine with success in his native country, he continued to pursue his favourite science with ardour. The discoveries which have the most contributed to his celebrity, are those which he made, in 1802 and in 1807, of the two small planets Pallas and Vesta, of which the first is so remarkable for the greatness of its inclination and for the eccentricity of its orbit, and the second for the intensity of its light, compared to the extreme smallness of its dimensions. These discoveries are so much the more remarkable because he only possessed a small observatory at the top of his house, and because it was while following up his theoretical ideas, that he was led to the discovery of Vesta, the last planet which has been recognised up to the present time. But the astronomical labours of Olbers were also extended to other subjects, in which he has rendered very important services to science. He occupied himself particularly with the theory and observation of comets; and he has given, for the determination of the elements of their orbits, a simple and convenient analytical method, published in 1797, at Weimar, under the care of the Baron de Zach, and which is still the most generally followed, especially in Germany. He was also much interested in the periodical appearance of falling stars, a subject which has excited great attention of late years; and his last letter, dated 19th August 1839, and published in Schumacher's Journal (Astronomische Nachrichten No. 384) related to that topic.* He preserved to a very advanced age his remarkable intellectual faculties and his scientific zeal. His character was modest, benevolent, elevated, and independent. His death was calm; and the Bremen Gazette, in announcing the mournful event, adds the following words, which embrace a simple but honourable encomium on this celebrated philosopher, in his public and his social relations. "During his long and brilliant career, he always enjoyed, as a medical practitioner, as a citizen, and as a man, the confidence, the grateful attachment, the esteem, and the friendship of his fellow-citizens."—(*Bibliothèque Universelle de Genève*, March 1840.)

Zoological Notices regarding the Cephalopoda, Pyrosoma, Salpa, Carinaria, Echinus, Holothuria, Hydrostatic Acalephæ, Beroe, and Coralline Polypidoms. By Messrs Milne Edwards and Peters.

1. *Cephalopoda.*—It is well known, that in the male apparatus of the Cephalopoda, there are certain singular bodies which were discovered by Swammerdam and Needham, and which, when they are extracted from the membranous pouch, where they are lodged side by side, perform various brisk movements, change their form, and speedily burst. Some naturalists have thought that these filiform bodies were sperma-

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* The 50th year of the doctorship of Olbers was celebrated at Bremen by a grand *fête*, the 28th December 1830. Numerous testimonies of esteem and respect were offered him on that occasion, both at home and abroad, and two medals, struck in his honour, were presented, the one by the Museum of Bremen, and the other by MM. de Zach and Lindenau.—(See the *Astronomisch: Nachrichten*, No. 192.)

tic animalcules of gigantic size, whilst others regarded them as parasitic worms. Their nature, however, was ill understood, and their use is altogether different, as we shall proceed to shew. MM. Milne Edwards and Peters have studied together all the Cephalopoda of which they could procure males. These were the *Octopus vulgaris*, the long armed *Octopus*, the *Octopus moschatus*, the *Sepia officinalis*, and the common Calmar (*Loligo vulgaris.*) In all these animals the spermatic filaments, or Needham's bodies, were encountered in abundance, and presented a very complicated structure. But there was always to be distinguished a case in form of a pod or husk, which was composed of two coats, and which inclosed a long tortuous tube like an intestine, filled with a white opaque matter, and connected with an apparatus which was more or less translucid. This intestine-looking tube is a spermatic reservoir containing thousands of zoosperms, and the apparatus to which it is attached at its anterior extremity, serves to burst the case, so producing the escape of the reservoir itself. The structure of the ejaculatory instrument and its mechanism varies in the different species. . . . . These bodies, which Cuvier called *the famous filamentary machines of Needham*, are neither spermatic animalcules, nor parasitic worms, but instruments of fecundation, such as, according to our knowledge, there is no similar example of in the animal kingdom. We propose to designate them *Spermatophoræ*, and they cannot so well be compared to any thing as to grains of pollen which likewise inclose fecundatory corpuscules, and which also burst, to discharge themselves when the male apparatus comes in contact with the female organs of the flower. In all probability, these spermatophoræ are, in the Cephalopoda, also a means by which the seminal liquor reaches the female apparatus, notwithstanding the absence of an external organ. Respecting the spermatic animalcules, which are inclosed in the interior of these singular bodies, they differ in no respect from those of other animals.—*Milne Edwards and Peters.*

2. *Circulation of Blood in the Pyrosoma.*—I have recently collected in the Bay of Villefranche, a small specimen of the singular aggregation of animals so well described, first by Peron and Lesueur, and afterwards by M. Savig-

ny, under the name of Pyrosoma. Having preserved it in sea-water, I could examine it in a living state. I had previously studied some of these Tunicata preserved in alcohol at Paris, but then, they have not that crystalline transparency which they present during life, and I could acquire but an imperfect idea of their organization. Nothing can be more curious than to observe the respiratory apparatus of these small animals, when the vibratile ciliæ with which each of the branchial openings is furnished, move all at once and turn about with extreme rapidity and perfect harmony.

But what proved most interesting to me, was the manner in which the circulation of the blood is effected among the *Pyrosomas*. The heart, which I believe has hitherto escaped the researches of anatomists, is placed in the lower part of the body at the side, and below the mass of the viscera; its disposition is analogous to that of the *Ascidias*. It contracts likewise in a peristaltic manner, and here the direction of this vermicular movement still changes periodically. The direction of the circulating current itself also changes periodically, quite in the same manner as among the Ascidias, and, as in these, the same vessels execute alternately the function of arteries and veins.

Such, then, is the anomalous mode of circulation established in all the great natural divisions of the Tunicata of Lamarck. It has been interesting to me to perceive, that a physiological phenomenon of so remarkable a kind, and which has not hitherto been observed in any other type of the animal kingdom, is not wanting in any of the animals composing the group intermediate between the true mollusca and the polypes.—*Milne Edwards.*

3. *Circulation and Nervous System in the Salpa.*—Since I made researches on the circulation of the Pyrosoma, I have had occasion to study the same function among the *Salpa*, and I am certain that the description which authors have given of it is far from being correct. I have likewise ascertained the existence of a nervous system in these animals, a fact which had escaped M. Savigny, and which, I believe has not been remarked by any other anatomist.—*Milne Edwards.*

4. *Carinaria.*—All the Mollusca of the order Heteropoda

have hitherto been considered as hermaphrodites; nevertheless we have found no difficulty in satisfying ourselves, that in these mollusca the sexes are perfectly distinct. The males and females even differ by the most evident external characters. In fact, in the males, we discover on the right side (the animal lying on its abdomen), underneath the visceral nucleus, a very distinct copulating apparatus, an organization which is wholly wanting in the female; but she, on the other hand, presents near the anus a genital organ of which the male is destitute. The testis occupies the same position as the ovaria, and very much resembles it, but instead of ovules characterized by the presence of a vitellin sac and a vesicle of Purkinje, it contains membranous capsules full of zoosperms. These animalcules have a very long tail, and are very rapid in their movements. The same observations apply to the *Pterotrachea.*—*Milne Edwards and Peters.*

5. *Echini.* (*Les Oursins.*)—Males and females perfectly distinct, also exist among these animals. Externally the testicles of these Echinodermata differ in nothing from the ovaries; but the liquid they enclose is white like milk, instead of being of an orange colour as in the females: and crowded with zoosperms whose tail cannot be seen without difficulty, although their movements are quite characteristic.—*Milne Edwards and Peters.*

6. *Hydrostatic Acalephæ.*—M. Edwards states, that he is convinced that these singular productions, which have been drawn under the name of Physophores, and which resemble long garlands of flowers intermixed with roundish bay leaves and stipules spirally contorted, are not simple animals; but on the other hand, aggregations of a great number of individuals produced by buds, and living united together as a compound polypidom. It is also probable that they possess a distinction of sex, for in some, in which it was impossible to perceive any trace of ovaries, organs were discovered full of spermatic animalcules.—*Milne Edwards and Peters.*

7. *Holothuria.*—The circulation in these creatures differs from the description given by M. Tiedemann, and very nearly approaches to the sketches given of it by M. Delle Chiaje.

8. *Circulation in Beroe ovatus.*—In this medusa there exists a

double system of vessels much developed, so that the circulation can take place in a complete manner. In certain circumstances the current is very rapid ; but there is nothing that can be compared to a heart, and the circular movement is determined by the vibratile ciliæ which cover the inner side of the vessels situate at one extremity of the system. This, as will be seen, is a mode of circulation of which we have as yet no example.—*Milne Edwards.*

9. *Coralline Polypidoms.* (*Polypiers coralligènes.*)—The stony polypidoms, of which M. de Blainville has formed his genus Dendrophyllia, have a structure both external and internal, which differs but little from that of the Actinia, and especially of the Caryophyllia properly so called. They possess, as do the animals higher up the scale, distinct sexual organs. Some are provided with ovaries, whilst others possess in the usual situation of the female organs, testes of the same form as these last, and inclosing spermatic animalculi, instead of ova.—*Milne Edwards and Peters.*

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany.

10*th June* 1840.

Bossiæa tenuicaulis.

B. *tenuicaulis ;* ramis teretibus, diffusis, filiformibus, foliisque ovatis subacutis rigidis mucronulatis, adpresse pubescentibus.

DESCRIPTION.—*Shrub* procumbent, branches long, slender, straggling, round, leafy, with adpressed pubescence. *Leaves* subsessile, ovate, subacute, mucronulate, dark green above, paler below, rigid, denticulate and recurved in their edges, having adpressed pubescence on both sides, slightly bullate above, middle rib and reticulated veins prominent below. *Stipules* filiform, marcescent, reflected. *Flowers* solitary, axillary, pedunculate, crowded into pseudo-spikes at the extremities of the branches. *Peduncles* rather shorter than the leaves, with two minute subopposite bracteolæ above the middle. *Calyx* glabrous, keeled along the upper side, bilabiate ; the upper lip divided into two broad rounded lobes, each with a tooth at its outer edge ; lower lip of three small revolute ovate acute teeth. *Corolla* (7 lines across) handsome ; vexillum reflected, slightly revolute in its sides, kidney-shaped, notched, yellow, behind and in the throat streaked with red, claw obconical ; alæ half as long as the vexillum, elliptico-obovate, yellow streaked with red, claws very slender; keel as long as the alæ, boat-shaped, blunt, dark red, its petals only connected with each other in the middle, and there slightly, claws slender and colourless. *Stamens* monadelphous, included within the keel; tube slit along its upper part, pale red; anthers ovate, yellow; pollen pale orange-coloured. *Pistil* as long as the stamens, stipitate, glabrous; germen compressed laterally; ovules about three.

This plant was raised at the Botanic Garden, Edinburgh, from Van Diemen's Land seeds sent by Mr Cooper, Wentworth House, in April 1836, and flowered for the first time in the greenhouse in March 1840. It seems to flower abundantly.

Cystanthe.

GENERIC CHARACTER.—Calyx foliaceous. Corollæ clausa, calyptræformis, transversim dehiscens, basi truncata persistenti. Stamina hypogyna, persistentia. Squamulæ nullæ hypogynæ. Capsula placentis ab apice columnæ centralis pendulis, salutis.

Frutex habitu Springeliæ, Ponceletiæ, et Cosmeliæ, nisi quod rami dum denudati annulati sunt.—*Brown*, Prodr. i. 555.

Cystanthe sprengelioides, *Br.* l. c.

DESCRIPTION.—*Shrub* and its branches erect, glabrous, rigid. *Leaves* scattered, crowded, sessile, stem clasping, ovate, acuminate, rigid, mucronate, reflected, serrulate, shining, persisting, many-nerved, nerves diaphanous. *Flowers* solitary, axillary, but crowded into capitula at the extremities of the branches. *Bracteæ* several, erect, distichous, in other respects resembling the leaves but smaller, and gradually passing into the submembranous *sepals*, which are still smaller. *Corolla* longer than the calyx, pale greenish-yellow, opening by circumcision about the middle, the upper portion crispid at its lower margin, and rising like a calyptra upon the summit of the elongated style, the lower part marcescent, marked by brown nerves. *Stamens* 5, hypogynous; filaments as long as the corolla, approaching below, somewhat spreading above, flattened, subspathulate, but pinched laterally at the apex, crystalline upwards; anthers erect, attached by their back near the base, ovate, yellow, bisaccate at the base, cleft at the apex, and opening within the cleft; pollen yellow, granules minute, spherical. *Pistil* longer than the stamens; stigma with five minute points; style colourless, tapering upwards; germen green, 5-lobular, 5-locular, loculaments tumid and ragged at the top, surrounding the base of the style; placentæ suspended from the column at the apex of the loculaments, green, and naked on their outer-side, but covered with suspended ovules on their inner.

This very curious shrub was discovered in Van Diemen's Land by Mr Brown. The specimen described (now 3½ feet high, and growing freely), was raised in 1836 at the Botanic Garden, Edinburgh, from seeds obligingly communicated in the autumn by N. B. Ward, Esq. London. As Mr Brown notices only one species of this genus, and gives no description excepting what is contained in the excellent generic character, I cannot be certain that this is the species which he saw; nor, if it be, have I any means of ascertaining to which of the varieties mentioned by him it belongs.

Janipha Lœflingii.

J. *Lœflingii*; foliis cordatis, 5-partitis, laciniis acuminatis integerrimis intermedia panduræformi, floribus racemosis.

Janipha Lœflingii, *Humboldt*, Nov. Gen. 2. 85.—*Spr.* Syst. Veget. 3. 77.

Janipha frutescens Lœflingii, It. 309.

Jatropha Janipha, *Linn.* Mant. 126.—*Willd.* Sp. Pl. 4. 563.—*Pers.* Synops. 2. 587.

Jatropha Carthagenensis, *Jacq.* 256. t. 162. f. 1.

β multifida, foliis 5-13-partitis, circumscriptione suborbiculatis.

DESCRIPTION.—*Shrub* (10 feet high in the specimen described) erect, everywhere glabrous, bark on the stem brown, cracked, and exfoliating; branches spreading, twigs pruinose, juice milky. *Leaves* (6–13 inches across) petiolate, glaucous behind, darker and more green in front, palmate, 5–13-partite, in circumscription suborbicular, segments obovato-cuneate, the outer the smaller, many of the middle ones often fiddle-shaped, quite entire in the edge, apiculate, each with a strong middle rib very prominent behind, slightly so in front, with many oblique, obscurely reticulated, pale, and subdiaphanous veins. *Petioles* spreading or pendulous, pruinose, round, considerably longer than the diameter of the leaf, having a large quantity of pith in its centre, and terminated with a tuft of short colourless hairs on the level of the front of the leaf. *Stipules* long, straight, erect, subulato-filamentous, cut in their edges, caducous. *Racemes* terminal, clustered, many-flowered, unilateral, spreading. *Flowers* monœcious, cernuous. *Bracteæ* 2, subopposite above the base of the pedicel, about equal to it in length, similar to the stipules, but smaller, entire and marcescent. *Perianth* single, glabrous, coriaceous, yellowish-green, with ten broad longitudinal red stripes about the middle, connivent in pairs towards their upper extremities. *Male flowers:* Perianth campanulate, 5-cleft, 10-ribbed, segments ovate, slightly overlapping, blunt, revolute, the ribs passing alternately to their apices and towards the sinuses, on approaching which they are cleft, and being afterwards repeatedly subdivided, they pass in scarcely reticulated lines along the adjoining edges of two lobes. *Stamens* 10, hypogynous: filaments alternately shorter, more slender, connivent, scarcely reaching as high as the sinuses of the perianth, alternate with the lobes; the longer stamens opposite to the lobes, following the course of the tube, adpressed but not adherent to it, and reaching nearly to the middle of the lobes; anthers yellow, oblong, bilocular, emarginate at both extremities, the loculaments on the inner side separated by a broad connective, on the outside nearer each other, opening along the sides; pollen granules rather large, spherical. *Disk* large, hard, orange-coloured, flat and depressed in the centre, marked round the edges with ten notches for the passage upwards of the filaments, which are inserted below it. *Female flowers* fewer than the male, expanding before them, placed at the lower part of the raceme; perianth single, pentaphyllous, caducous; foliola ovato-lanceolate, rather unequal (the two inner being a little smaller than the three others), blunt, spreading wide, undulate, revolute in their edges in their upper half, concave in their lower half, or there sub-bisaccate and nectariferous, each with three ribs, and several smaller ones towards the edges. *Stamens* altogether awanting. *Disk* as in the male flowers, but only indented on its lower side by the ribs of the perianth. *Stigmata* large, multifid, colourless, orbicular-reniform, the sinus on the upper side. *Styles* 3, short, united at the base. *Germen* sessile in the centre of the disk, green, glabrous, oblong, little more than half the length of the perianth, 3-celled, 3-lobed, the lobes becoming obscure as the germen enlarges. *Ovules* solitary, pendulous by a curved fleshy stalk from the inner angle of the cell.

Seeds of this shrub, whose foliage is remarkably handsome, and the flowers not without beauty, were received at the Royal Botanic Garden, Edinburgh, in 1830, from Mr Dick of Irvine. They were obtained by him from Mr Tweedie, but in what part of South America they were collected I cannot learn. The plant has been cultivated in sandy peat soil, and kept in the stove, where it flowered abundantly at the end of almost every branch in the end of May 1840, and again towards the middle of June, before the first crop had wholly disappeared. It is not probable that the plant will be long-lived, though at present in great vigour, but there is every reason to believe that it will ripen seed.

In a genus known to vary so much as *Janipha* does, I have thought it better to consider the plant now described as a variety of *J. Lœflingii*, rather than one entitled to rank as a species. It may be right, however, to put in contrast characters which Jacquin states to belong to the plant which he describes, and those which belong to our plant.

Janipha Lœflingii.

α.	β multifida.
Longer stamens equal to the corolla.	— reaching nearly to the middle of the segments.
Flor. fœm. filaments 10, shorter than the germen.	— not a vestige of filament.
Germen with 6 blunt angles.	— with 3 rounded lobes.
Leaves perfumed like walnut.	— less strongly and less agreeably.
Leaflets five.	— five to thirteen.
Petiole 6 inches long.	— of larger leaves above 13 inches.
Stem scarcely branched.	— many spreading branches.
Juices watery.	— milky.

Pimelia nana.

P. *nana*; foliis alternis, rarius suboppositis, spathulato-linearibus, utrinque pilosis, uninervibus, floralibus rameis similibus; capitulis terminalibus, multifloris; stylo staminibusque limbo perianthii æquantibus.

Description.—*Stem* (8 inches high) woody, slender, erect, with alternate ascending branches, its whole surface, excepting the inside of the corolla, the stamens and style, covered with soft white and spreading simple hairs, mixed with others which are much shorter and more dense. *Leaves* (½ inch long, about 1 line broad) scattered, spathulato-linear, glaucous, rather paler and more hairy below than above, spreading, flat, with a conspicuous middle rib, but no lateral nerves. *Capitulum* terminal, many-flowered; involucre similar to the stem-leaves. *Perianth* white; tube green where it covers the germen, rather longer than the involucre, slender, having long spreading hairs externally, and only short dense, hairs within; limb somewhat irregular, segments imbricated, the upper and lower outermost, ovato-lanceolate, glabrous on their upper surface, the two lateral ones deflected. *Stamens* as long as the perianth; filaments glabrous, at first erect, afterwards spreading along the upper and lower segments of the limb, white; anthers brown, rhomboideo-oblong, attached by their back near the base, bursting along their sides: pollen granules spherical, of the same colour with the anthers. *Stigma* blunt. *Style* glabrous, subterminal, exserted, as long as the stamens. *Germen* pale green, ovato-oblong, with a small tuft of erect hairs on its apex, elsewhere glabrous. *Ovule* solitary pendulous, filling the germen.

We received this plant at the Botanic Garden, Edinburgh, in 1839, from Mr Low of Clapton. It is a native of the Swan River settlement in Australia, and flowered abundantly in the greenhouse in the end of April, and in May and June. It is allied to *P. longiflora*, Hooker, Bot. Mag. 3281, but is easily distinguished from this by its much more humble growth, its single-nerved leaves, and its exserted stamens and style. It will probably be short-lived.

Verbena tuberosa.

V. *tuberosa*; radice tuberifera; caule suffruticosa, erecta; foliis petiolatis, ovato-ellipticis, utrinque obtusis, inciso-serratis; spicis terminalibus, solitariis, floribus verticillatis.

Description.—*Root* perennial, creeping, the fibres having many rounded tubers about the size of filberts. *Stem* herbaceous, branched, erect, square, green, slightly glutinous. *Leaves* opposite, decussating, petiolate, concave in front, slightly rough, inciso-serrate, ovato-elliptical, of bright green, paler below where the midrib and veins are very prominent, channelled above. *Petiole* more than half the length of the leaf below, shorter upwards. *Flowers* in terminal, verticellate, solitary spikes; pedicels very short, each with an ovate, undulate leafy *bracte* below its base. *Calyx* curved, with five blunt but prominent angles, coloured above, green below; teeth five, subulate, unequal, the lowest the longest. *Corolla* flesh-coloured, agreeably perfumed; limb very oblique and very unequal, above glabrous, except towards the throat, where, especially on the lower side, it is pubescent, below very slightly hairy; lobes obovato-linear; tube curved, scarcely longer than the calyx, hairy both within and without, excepting where inclosed within the tube of the calyx, where, both within and without, it is glabrous. *Stamens* very unequal, the longer rising from the lower side of the flower, and projecting into the faux; filaments hairy, those of the shorter stamens adhering to the middle of the corolla, the longer somewhat farther; anthers ovate; pollen granules small and round. *Pistil* rather shorter than the stamens, everywhere glabrous; germen ovate, green; style slightly curved, colourless; stigma bilobate, the upper lobe very small, the lower curved downwards.

We received this plant, without name, at the Royal Botanic Garden, from the garden of the Dublin Society, and understood it had been imported from Buenos Ayres. It flowered in the greenhouse. In the arrangement of the species it should probably stand next *Verbena platensis.*

Before I was aware of the tubers on the root, I had given this plant the specific name of *lobelioides*, and I believe it exists in various collections so marked; but as no account of it has been published, I do not hesitate to substitute the far more appropriate designation now given.

Proceedings of the Wernerian Natural History Society.

(Continued from vol. xxviii. p. 414.)

March 21. 1840.—Dr Traill, V. P. in the Chair.—Mr James Wilson read an account of Mr Shaw's experimental observations on the fry of the Salmon; illustrating the same by specimens of the fish in its various stages

April 4.—Professor Jameson, President, in the Chair.—Mr William Nicol stated the result of his observations on the optical properties of Greenockite. The specimen employed was a cleavage plate, extremely thin, and so narrow as barely to cover an aperture of about the thirtieth part of an inch in diameter. It was highly transparent, and of a deep yellow colour. On transmitting through it the polarized light of dullish clouds, or that of a deep blue sky, it did not appear to depolarize the light; but when placed between two single vision prisms of calcareous spar, and exposed to the bright light of a gas burner, or to that of bright white clouds, it depolarized light in the same manner as was done by similar minute portions of crystals possessing one axis of double refraction. On account of the extreme thinness and limited extent of the specimen, the coloured rings could not be brought into view.*

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* An examination of some additional specimens of Greenockite leaves little doubt of that mineral belonging to the rhombohedral system of crystallization.—Editor.

Mr Robert Bald, mining engineer, then read a paper regarding the edge coals, near Edinburgh. In particular, he noticed the position and great aggregate thickness of these coals in the lands of Duddingstone and Brunstain, the property of the Marquis of Abercorn. The coals in this property, bounded by the sea on the east, lie at an angle of about 65° with the horizon, dipping to the south. They consist of two distinct groups, viz., the Duddingstone group on the north, and the Brunstain group on the south. The North Green coal, which reaches within a few hundred yards of the town of Portobello, is the most northerly, and lies within a few fathoms of the mountain limestone. It yields the chief supply of parrot or pitch coal for the gas-works. The limestone encircles the whole of the great coal basin of the district, and is held as a sure index for the finding of the coals in the basin. The most southerly coal of the two groups is that named Greymechan, which reaches within a few yards of the flat coals in the valley of the Esk. The edge coals pass under the flat coals, and again rise along the elevated ground in the Dalkeith district, dipping to the north at an angle of from 13° to 18°. These are termed half-edge coals. The Duddingstone coal seams are twenty in number, and their aggregate thickness is sixty-eight feet. Those of Brunstain are six in number, and their aggregate thickness fifty-two feet. Hence these two groups of coal constitute together 120 feet thick of coal. Mr Bald mentioned, that the most striking geological feature of this coal-field, including the two groups, is this:—That the distance from the seven-feet coal in Brunstain, to the south parrot-coal in Duddingstone, is no less than 622 yards; that in this vast space there are only two thin beds of coal found, the one being merely two feet, and the other two feet seven inches thick. The author then shewed, that in each square yard of surface, measured parallel with the dip of the coals, and taking into account the aggregate thickness of the coals, there exist no less than thirty-six tons of coals; and that, therefore, less than two imperial acres would yield above 300,000 tons, equal to the whole annual consumption of Edinburgh, Leith, and the surrounding towns and villages. Upon these rich coals, collieries had been established centuries ago. About forty years ago, they were drained by powerful atmosphere steam-engines; but these being overpowered with water, were abandoned, and no coal has been wrought there for many years past. Now, however, the coal-fields have been let to a capitalist in London, who is much engaged in mines; and there is no doubt that the workings will forthwith be revived with vigour and efficiency.

Dr Traill next communicated some remarks on the food of the genus Trochilus, and stated, that having frequently dissected different species of Trochilus, which had been put up in spirit when recently killed, he had invariably found the expansion of their œsophagus corresponding to the crop of granivorous birds, to contain insects, and, in some instances, to be completely stuffed with them, among which he had never observed any apterous insect. In a specimen of Trochilus viridissimus, opened in presence of the meeting, two species of dipterous insects were found. From these observations he inferred, that Alexander Wilson and some other naturalists were not only right in asserting that insects were occasionally eaten by humming-birds, but that the chief object of their fluttering about flowers was more for the purpose of obtaining insect food than for the alleged object of sucking the honey from the nectaries of plants.

Professor Jameson directed the attention of the members to the height of the coast-line of fuci or sea-weeds; remarking, that important geological results would seem to have been obtained by the French northern scientific expedition, from observations made on the variations it presents in Norway, as mentioned in the April number of the Edinburgh New Philosophical Journal, p. 420. It was recommended that comparative measurements should be made on the coasts of Scotland and its islands, with the view of collecting information regarding a phenomenon which appears to bear so directly on the question of the supposed elevation of the land, or the depression of the sea. The Professor then exhibited the skin of a species of sheep from the lofty Himmalayan range, much resembling the argali of Siberia, but differing from the argali of North America. He observed, that if the animal could be introduced alive into Scotland, it would probably thrive in our Highlands, and become valuable, not for wool certainly—the fur being hairy and like that of the deer—but for its flesh, which is described as delicate and excellent for food.

April 18.—Dr ROBERT HAMILTON in the Chair.—Mr Edward Forbes communicated observations on certain rare British Zoophytes; illustrating his remarks by drawings.

Professor Jameson read a communication by Professor L. A. Necker on some rare Scottish minerals (published in this Number, p. 75). He likewise exhibited specimens of the lead and iron ores of Craigengillan, in the parish of Carsphairn and Stewartry of Kirkcudbright.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *On the Odour of the Electric Fluid.* By M. SCHOENBEIN. M. Schoenbein, at the beginning of May last, presented to the Royal Academy of Paris, a communication concerning the odour connected with the electric fluid, of which we shall now present a short account.

Being struck with the resemblance which exists between the odour which is developed when common electricity issues from the point of a conductor to the surrounding air, and that which is disengaged when water is decomposed by the galvanic current, M. Schoenbein instituted a series of experiments, on the results alone of which we shall now dwell. The following are the facts, and, according to the author, they afford a complete explanation of the phenomenon.

1*st.* The phosphoric odour which is developed during the electrolyzation of water, is disengaged only at the positive wire.

2*d.* The disengagement of the odoriferous principle, depends, 1*st,* upon the chemical nature of the substance which affords the positive electricity; 2*d,* Upon the chemical constitution of the fluid employed in the trough; and 3*d,* Upon the temperature of this same fluid. Regarding the first condition, the author has found that, of all the metals he has examined, gold and platinum alone produce the disengagement of the peculiar odour. Those metallic substances which are very readily oxydized, do not furnish the slightest trace of it, and charcoal, which is an excellent conductor, also belongs to this category. Respecting the connection which exists between the fluids employed, and their power of disengaging the odorous principle, the experiment of M. Schoenbein has demonstrated that the electric odour is developed at the positive wire, when the fluid consists of distilled water mixed with sulphuric, phosphoric, or nitric acids, or with potash, and a variety of oxydized salts. The odour, on the other hand, is not observable when the water contains chlorides, bromides, iodides, fluorides, the protosulphate of iron, or any substance whatever which has a strong affinity for oxygen. No more does it occur, if the fluids which have just been enumerated in the former of these lists are mixed even with small quantities of the protosulphate of iron, or with nitrous acid, or with any other substance whose affinity for oxygen is equally great. *Lastly,* Those fluids which abundantly disengage the electric odour at a low temperature, do so no longer when heated to their boiling point.

3d. The odoriferous substance which is disengaged from the positive wire, may be inclosed and preserved in a well-corked vessel.

4th. When into a vessel containing this substance, there is thrown a small quantity of powdered charcoal, or filings of iron, zinc, tin, lead, bismuth, arsenic, antimony, or a few drops of mercury or nitrous acid, or of a solution of protosulphate of iron, or of protochloride of tin or iron, the odour is nearly in a moment entirely destroyed; and at a high temperature, gold and platina produce the same effects. When a plate of gold or platina, whose surface is quite dry and free from tarnish and cold, is plunged into a jar containing this substance, the metal becomes negatively electrified, or, in other words, a plate of platina heated in this way, forms with another plate of the same metal in its natural condition, a voltaic element. The metals which are readily oxydized, are not negatively polarized in the same circumstances. The author, in the course of the previous year, had demonstrated that the precious metals assumed negative polarity when they are plunged for a little time into an atmosphere of chlorine or bromine. This negative polarity of platinum is destroyed when the plate is plunged for a few instants into an atmosphere of hydrogen.

M. Schoenbein regards the odorous principle which he has been examining, as a distinct body which ought to be classed with the kind of bodies to which chlorine and bromine belong, among elementary and halogenous substances. He proposes to give it the name of *Ozone*. He considers it certain, that this body is always disengaged in the air, and in very considerable quantities during the time of thunder storms.

2. *Polar Lights.*—Some of the labours of the French Scientific Expedition to high northern latitudes, have been lately submitted to the attention of the Royal Academy of Sciences, and from these interesting documents we now select a few particulars on *Polar lights*. Between the 12th of September 1838, and the 18th of April 1839, 153 appearances of the aurora borealis were observed; sometimes the phenomenon was noticed as early as half-past three in the afternoon, and sometimes it continued till five in the morning. It has often afforded light to read a printed book, and frequently of nearly the smallest type. Parallactic observations, which have been taken at Dupvig and Bossekop, the two extremities of a base eight and a half miles long, between the 9th and 22d of January 1839, seem to assign to those auroras which

were seen at this epoch, an inferior limit, notably higher than the result of the observations of Franklin and his companions. The particulars of its colouring have also been noted with care, and the results may be thus stated. The usual colour is a yellowish tint, which may become whitish or greyish, especially towards the conclusion of the phenomenon. The more striking exhibitions of colour are produced by red tints or green; and these are witnessed only in the most beautiful auroras, which occur, in comparison to the others, in the proportion of one to five. This kind of colouring seems to require, as a condition, great vivacity of splendour, and very rapid undulatory movements or vibrations. The mode of the distribution of the colours has also been studied, and is very remarkable. When an arch is very brilliant in a clear sky, it forms at the lower part a light reddish shade, and at the upper a very slight greenish tint, and the general shining light continues imprisoned between the two small coloured zones; ere long the rays appear; but this phenomenon, upon the whole, is far from common. Even when the vibratory movement is perceptible, if there be any shooting of the rays, the red tint occupies the base of the ray, the green the summit, and the yellow the intermediate space. In proportion as the splendour increases, the more the colours at the extremity extend, and at the expense of the colour between them. If the splendour diminishes, these colours retreat to the extremities of the rays, and then disappear. If, on the other hand, the ray is following an undulatory motion, and is moving along parallel to itself, of the two lateral faces one exhibits the red tint, and this is the anterior one, whilst the other and posterior one is of a green hue. These two tints have not at all appeared identical with their homonyms of the solar spectrum. Possibly the red aurora of our climates may be explained by the appearance of arcs or rays, whose lower part alone is visible to us. Three or four times the aurora borealis has been seen, apparently placed between the observer, on the one hand, and the clouds or the mountain snow on the other.

The influence of the aurora borealis upon the magnetic needle has been studied with the greatest care: that of the more brilliant ones was very conspicuous; the needle almost always traversed towards the west, then returned to its point of equilibrium, and advanced towards the east, and did not definitively return to its original position, till after a series of oscillatory movements which were usually very irregular. The maximum deviation observed has been 4° 30′, and this on the evening of the 22d of February; and it is especially during the appearance of coronas that these striking deviations appear. Those examples of the aurora borealis which are not brilliant, whose glare is diffused, or which do not leave the northern horizon, have, on the contrary, little influence upon the magnetic needle. Finally, there has been no instance during the prevalence of a pure sky, and free from the aurora during the course of the night, which has coincided with a magnetic agitation of any extent, a fact which goes to support the opinion that all the incessant and irregular perturbations of the needle (with the exception of the regular, daily, monthly, and annual variations) are owing to the aurora borealis.

Major Sabine, in a Report on "Observations made on the Aurora Borealis at Alten, in Finmark, by Mr S. H. Thomas," just laid before the Royal Society, remarks—"On examining Mr Thomas's Register, with a reference to Mr Erman's important remark, that 'in Siberia two kinds of aurora are distinguished, one having its centre in the west, and the other in the east, the latter being the more brilliant,' I find that twenty-eight nights occur in the course of the two winters, viz. 1837 and 1839, in which the formation of arches of the aurora is noticed, and their direction recorded; of these *ten* are to the *west*, having their centres rather to the southward of west, the arches extending from NW. to S.S.E. and SE.; *seven* are to the *east*, or more precisely to the southward of east, the arches extending from NE. to SE. and SW.: of the five others, *four* are said to be from east to west across the zenith, and cannot, therefore, be classed with either of the preceding; and *one* is noticed generally as being to the north. The facts here recorded appear to afford an evidence of the same nature as those mentioned by Mr Erman, as far as regards there being two centres of the phenomena. In respect to the relative brilliancy of the eastern and western aurora, nothing very decided can be inferred from the register. If, as Mr Erman supposes, that they may be referred respectively to 'les deux foyers magnétiques de l'hemisphère boreal,' it is proper to notice that the position of Alten is nearly midway between these localities. There can be no doubt that the frequent appearance of the aurora, and the peculiarities of the phenomena observed there, render it a most desirable quarter for a magnetical and meteorological observatory."

3. *On Terrestrial Temperature.*—Mr Dove lately read to the Berlin Royal Academy a memoir upon the changes of form which the isothermal lines undergo during the course of the year, of which we present the following notice:—

The immense influence which currents of air exercise, and the fall of the temperature of the place which accompanies them, do not, as is known, manifest themselves in the course of a single year, to such a degree as very sensibly to affect the temperature as compared with that of another year. The mean monthly variations of a single year are infinitely more considerable. For the purpose of estimating them with certainty, a series of annual observations is required, such as is, as yet, collected for a very small number of places. There are, therefore, for the construction of lines of equal monthly heat, out of the observations which we possess, three corrections to be made; 1st, To free the observations which extend to too short periods of the accidental deviations of the true means; 2d, the elimination of the daily variations; and 3d, The reducing the observations to the sea-level.

The researches which were submitted the preceding year to the Academy, upon the extent which the same meteorological phenomenon embraces, have demonstrated, that the wide differences of different mean temperatures, are never a local phenomenon, but extend simultaneously over a large surface of the globe. We may hence then conclude, that the deviations that are remarked in a given place, will be observed also in another neighbouring place. In the first of the tables formerly printed, there was calculated the deviations of each month of the interval between the year 1807 and 1824, upon the general means during that interval for the following places, viz., Madras, Palermo, Nice, Milan, Geneva, Carlsruhe, Stuttgard, Munich, Ratisbon, Paris, Placentia, London, Carlisle, Dunfermline, Berlin, Dantzic, Stockholm, Torneo and Salem. Let us suppose, for example, a spot placed in the neighbourhood of Nice, and for which we are furnished with observations from the year 1810 to 1815, it may be understood, that the difference of the monthly mean of Nice in the same interval of 1810–15 and that from 1807 to 1824, may be employed as the element of correction for the observations made in this neighbouring locality. By the combination of five different systems of analogous observations, simultaneous in the interval of fifty years in the district which embraces them all, it becomes possible to eliminate the non-periodic and purely fortuitous changes, whilst the corrections dependent on the daily period, are calculated by particular tables prepared for this purpose for every month. The reduction, however, to the sea-level has presented the greatest difficulties on account of its many varieties and its local circumstances.

The final result of these researches may be expressed in the following terms: The cold poles of the earth, which, in the depth of winter, are at their greatest mutual distance, and from the common *pole of rotation*, in the summer approach nearer and nearer to each other, till they almost appear to unite, or the space between them is upon a line placed at a right angle to the primitive line which united them.

The isothermal lines, consequently, are composed like the isochromatic curves of certain crystals, when their temperature is gradually raised. At the same time, the observations of high latitudes are as yet too general to enable us to decide definitively on this question.

At certain epochs of the year, accordingly, the earth has only one cold pole. The isothermal lines of the mean zone turn so much in their progress, that in some countries they become perpendicular during half the year, in relation to the direction they maintained during the other half. We are unacquainted with any parts of the earth, whose temperature does not fall, during some of its months, below the freezing point; and there are places whose temperature is so low, that the mean of a whole month descends below the point which freezes mercury.—*L'Institut,* 8 an. No. 325, p. 106.

4. *Mirage in New Holland.*—There was one other district examined by us, which possesses such peculiar characteristics, that even in this short report I am induced to call your Lordship's attention to it. I have named this the district of Koo-heni-triet, that is, the district of Falsehood or Deceit. It is situated between a point lying about ten miles to the north of the northern mouth of the Gascoigne and Cape Cuvier. The whole extent of its seacoast is bounded by a range of lofty sandy dunes, having a width inland of not more than from two to two and a half miles. The first time that I ascended this range, was on the morning of the 8th of March 1839, at a point about fifteen miles south of Cape Cuvier. On looking to the eastward, I was surprised to see an apparently boundless expanse of water in that direction. I hurried back to the boats, and selected three men to accompany me in my first examination of the shores of this inland sea. When we gained the top of the sandhills, the surprise of the party was as great as my own, and they begged me to allow them to return, and endeavour, by the united efforts of the party to carry one of the whaleboats over the range, and at once launch it on this body of water. I, however, deemed it more prudent, in the first instance, to select the best route along which to move the whale boat, as well as to choose a spot which afforded facilities for launching it. In pursuance of this determination, we decended the eastern side of the sandhills, which abruptly fell in that direction, with a slope, certainly not much exceeding an angle of 45°. I now found that the water did not approach so near to the foot of the hills as I had at first

imagined, but that, immediately at the foot of these hills, lay extensive plains of mud and sand, at times evidently flooded by the sea, for on them lay dead shells of many kinds and sizes, as well as large travelled blocks of coral. The water now appeared to be about a mile distant; it was apparently boundless in an easterly and north-easterly direction, and was studded with islands. We still felt convinced that it was water we saw, for the shadows of the low hills near it could be distinctly traced on its unruffled surface. As we continued to advance, the water, however, constantly retreated before us, and at last surrounded us. We had been deceived by mirage!—The delusive appearance of water of the desert.

GEOLOGY.

5. *Agassiz' Tour to the Swiss Glaciers.*—After the Bern meeting, I went with Studer to Monte Rosa and to the Matterhorn, and afterwards, when he proceeded to Piedmont, I visited the Aletsch glacier behind the Jungfrau, then the Rhone glacier, and finally extended my expedition as far as Hugi's hut on the Aar glacier. I found my previous observations everywhere confirmed, and intend publishing a full account of the results of my tour. I have had sketches drawn by a skilful artist who accompanied me, so that I shall be able to give faithful representations of all the phenomena. The most important new fact which I have observed is, the forward movement of Hugi's hut. Since the year 1827, it has advanced upwards of 4000 feet. In 1830, Hugi found it some hundred feet from the spot where it was built; in 1836, it was above 2000 feet from it; but this year I found it removed to a distance of 4000 feet from its original position, and still the glacier is very little inclined, and the mass pressing from behind is very inconsiderable compared with the lower mass, so that the explanation of the progressive movement of glaciers, by sliding and pressure from behind, goes for nothing. I recommend every one who wishes to learn any thing of glaciers, to examine more particularly the *Lower Aar* glacier and the glaciers between *Monte Rosa* and the *Matterhorn*. But the *Aletsch* is the most imposing, on account of its great size and the frightful desolateness of its vicinity. Nothing can surpass the polish of the Serpentine over which the Gorn glacier is advancing or has advanced. Hugi's hut proves, in the most distinct manner, that the deeper the glacier the more rapid is its movement. During this journey, my friends Desor and Nicolet likewise accompanied me, so that the observations were not merely made but were fully discussed. I think that I could now convince *any one,* who would accompany me for a few weeks to the Alps, *of the correctness* of my views; and as I propose visiting them

every year until I have distinctly made out every thing about the matter, friendly society were much to be wished on a tour which cannot well be made alone.—(*Letter from M. Agassiz in Leonhard and Bronn's Jahrbuch, dated September* 18. 1839.)

6. *Origin of Fissures in Glaciers.*—At a recent meeting of the Philomathic Society of Paris, M. Martins read a memoir on the glaciers of Spitzbergen, compared with those of Switzerland and Norway. This communication gave rise to a discussion on the nature of the cause which produces the fractures in glaciers. M. Martins attributes them to the unequal dilatation undergone by the different parts of these masses of ice, combined with the manner in which they are enclosed by the surrounding mountains. M. Elie de Beaumont defended the opinion of geologists, who consider the sliding of these masses of ice on inclined planes as the principal cause of the fractures. He remarked, that the rents are precisely in the direction of this sliding, that is to say, they are in a direction perpendicular to the axis of the valley, whereas they would be perfectly irregular if they were produced solely by unequal dilatation in the different parts of the glacier. M. Martins replied to this observation, that, first of all, imperceptible fissures are formed in all directions, but that those having a transverse direction alone present the conditions necessary for enlargement; that, undoubtedly, the weight of the ice above contributes to the phenomenon, but that the greater part of the action ought to be attributed to the water which falls into the fissures, and which, becoming consolidated, acts as a wedge on their walls.

7. *Sefström's Investigations.*—Sefström continues his observations on the furrows on rocks. The return is expected of one of his scholars, who is occupied with similar investigations in the northern portions and on the sea-coast of Norway. I cannot coincide with the objections which have been made to the views of Sefström. Although great rushes of accumulations of water have produced the boulders and furrows of the Swiss valleys, yet this only proves that boulders and furrows are produced exactly in the manner assumed by Sefström. But we cannot thence draw the conclusion, that the enormous rolled masses, and the almost unvaried direction of the furrows, throughout the whole of Scandinavia and Finland, owe their origin to such comparatively small accumulations of water as exist in Switzerland. The high position of such accumulations is entirely awanting in Scandinavia: and how prodigious a mass of water it must have been, that polished and furrowed over all Sweden, hills of more than 300 feet in height.—*Berzelius, in Leonhard and Bronn's Jahrbuch.*

8. *Geological Constitution of North-eastern Africa.*—The following is a brief statement of the results obtained by Russegger, from his geological investigations made in the north-eastern portion of Africa, from the Mediterranean to near the Equator, and of which a full account is given in a recent number of Leonhard and Bronn's *Jahrbuch.* 1. Chalk and tertiary formations. Plains and mountainous deserts. The cultivated land of the Nile and of the Oases. *Egypt.* From the 31st to the 26th degree north latitude. 2. *Muschelkalk* and *bunter* sandstone. Mountain-chains of abnormal rocks. Plains and mountainous deserts, and, in the southern portion, a transition to the Savannahs. The cultivated district of the Nile. *Egypt, Nubia.* From the 26th to the 15th degree. 3. *Bunter* sandstone and alluvial formations. Rarely outbursts of abnormal rocks, although they form the fundamental basis of the country. Savannahs. Plains north of *Kordofan* and *Senaar.* 15th to 13th degree. 4. Granite and porphyry forming isolated mountain groups in plains which are covered by alluvium. Savannahs. Plains 1100 feet above the sea. South from *Kordofan* and *Senaar, Roserres.* From the 13th to the 12th degree. 5. Granite, gneiss, mica and chlorite slates of the Alps, with auriferous alluvium. Mountain land south from *Kordofan* and *Darfur.* Great plain of the *White River.* High mountain land on the *Blue River.* Ethiopian high land. The *Plateaus* reaching to a height of 3000 feet, and the mountains to a height of 7000 feet, above the level of the sea. Presenting the animal and vegetable development peculiar to the tropics. The country of the *Nubas,*—the plains of the *Dinkas, Fassokl,*—the country of *Berta,*—the country of the *Gumuss* and the *Galla.* From the 12th to the 10th degree. 6. Extensive unknown plains south from *Nuba,* and in the countries of the *Dinka* and *Galla,* traversed by large rivers. Savannahs. Beyond the 10th degree north latitude.

9. *Distribution of the Mammoth in Siberia.*—Without entering in this place into any speculations concerning the manner in which these probably antediluvian remains came into their present situations, I would call attention to the remarkable fact, that the teeth, tusks, and bones, which are called by the general name of Mammoth bones, but which probably belong to several different species of animals, are not distributed equally over Siberia, but form immense local accumulations, which become both richer and more extensive the farther one advances to the north. They are found in the greatest abundance in New Siberia and the Lachow Islands, as mentioned by Reschetnikow and Sannikow. Many hundred pood (*pood* equal to 36 lb. avoirdupois) weight are collected there every

year, whereas, on the Continent, they are much scarcer, and are hardly ever met with in the southern part of Siberia.—*Wrangell*, p. 185.

10. *Dysodolite.*—This mineral, arranged in systems of mineralogy under the name *Foliated Mineral Pitch*, Ehrenberg has shewn to consist of bitumen, or mineral pitch, mixed with siliceous shells of infusoria, and occasionally with pollen of pines, &c. The wax-yellow variety found in Sicily is made up of shells of navicula and mineral pitch: the nearly black-brown coal of the Westerwald is a variety of dysodolite, so also is the foliated leather-like bituminous coal of the Gesteinger Busch at Rott and Siegburg in the Siebengebirge, and a foliated brown coal from the Vogelsberge. Hence the mineral species named dysodolite appears to be a polier slate, impregnated with bitumen. Its colours are black, brown, or black. It never forms very thick beds, but sometimes widely spread deposits. It is used as fuel.

11. *Pihlite, a New Mineral.*—Sefström has discovered at Fahlun a new mineral, which replaces mica in granite, and which he has termed Pihlite in honour of the late M. Pihl, director of mines. It is an intermediate substance between talc and mica, and its composition is expressed by the following formula:

$$\left.\begin{matrix}\mathrm{M}\\ mn\end{matrix}\right\} \mathrm{S}^2+3\left.\begin{matrix}\mathrm{A}\\ \mathrm{F}\end{matrix}\right\} \mathrm{S}.$$

—*Berzelius, in Leonhard and Bronn's Jahrbuch.*

12. *Pyrrhite, a New Mineral.*—Only one example of this substance is known, and occurs in a splendid drusy cavity of felspar, which is in the possession of Vice-President Perowski of Petersburgh. While the cavity chiefly contains felspar crystals several inches in size, finely defined, and of an ochre-yellow colour, it likewise includes six-sided tables of reddish-white, pearly lithion mica; white translucent crystals of albite; crystals of clove-brown rock-crystal; and a few white topazes. The crystals of the new mineral are superimposed on the felspar, are eight in number, and are octahedrons of about three lines in length. Their surfaces are smooth, but possess little lustre, so that their angles cannot be measured with great accuracy; but, from observations made on several angles, the mean may be regarded as 109° 28′, so that we may probably assume that the crystals are regular octahedrons. No cleavage is observable. The colour is orange-yellow, and the lustre feebly vitreous. The substance is translucent on the edges; its hardness is that of felspar, but the specific gravity could not be de-

termined. It occurs at Alabaschka near Mursinsk, and, on account of its yellow colour, has been named Pyrrhite.—*G. Rose in Poggendorff's Annalen.*

ZOOLOGY.

13. *On the Zoological Labours of M. Sars, of Floröe in Norway.*—In the high north, beyond the 60th degree of northern latitude, there is a clergyman at Floröe, almost entirely cut off from the learned world, and distant about 280 miles from a zoological library, who, by his indefatigable zeal, has made himself acquainted with all the most important zoological discoveries, and has himself increased the number of these discoveries in a most remarkable manner. It is the sea animals of the lower classes which he accurately and judiciously describes, and admirably represents. In 1835, he published his work entitled *Beskrivelser og Jagttagelser over Nogle Maerkelige eller nye i Havet ved den Bergenske Kyst levende Dyr, af M. Sars,* and which is illustrated by fifteen plates. Two things are to be regretted; that he writes in the Danish language, and thus prevents the diffusion of his discoveries, and that the lithography of Bergen is not capable of producing the sharp outlines which are necessary in minute zoological objects. He previously published a contribution to the natural history of marine animals in 1829, and an account of it was given in the *Isis* of 1833. In his last published work, mentioned above, he has improved much of what he formerly made known: but the greater portion of it is quite new, and at the same time very instructive. His observations were made between 60° and 60½° north latitude, and the subjects of them have been the Polypi, Acalephæ, Asteridæ, Annulata, and Mollusca.

Most of the species described are figured both entire and in their different parts. The descriptions are very full, and are made with a perfect knowledge of the most recent investigations in the same field, so that the determinations are to be depended on. This remarkable book contains several new genera and many species, some of which really fill up gaps in the orders of the zoological system. It is much to be wished that the active author were placed in a locality more suited to the cultivation of the sciences, and in one more congenial to his nature and acquirements, such as Christiania or Copenhagen.—(*Abridged from Oken's Isis.*)

14. *Aristotle's History of Animals.*—Dr Osborne, in a memoir read before the Royal Irish Academy, commenced by observing, that this work was composed under circumstances more favourable to the acquisition of natural knowledge than any work on the subject ever published. According to Pliny, some thousands of men

were placed at the disposal of the author, throughout Greece and Asia,—comprising persons connected with hunting and fishing, or who had the care of cattle, fish-ponds, or apiaries,—in order that he might obtain information from all these quarters, *ne quid usquam gentium ignoraretur ab eo.* And according to Athenæus, the same prince gave him, on account of the expenses incurred in composing it, 800 talents,—a sum which, taken at the lowest, that is, the lesser Attic talent, amounts to above L.79,000. The work composed under such auspices, is such as might have been expected. The extent of the observations is prodigious; and we cannot read far in any part of it, without being constrained to exclaim with Cicero, *Quis omnium doctior, quis acutior, quis in rebus vel inveniendis vel judicandis acrior Aristotele?*

Shortly after the introduction of Greek literature to Europe, and when this book was first printed, those sciences which have nature for their object were in the lowest condition. There was at that time no taste diffused for the study of zoology or comparative anatomy; and at later periods, when the value of these studies came to be better appreciated, the Aristotelian philosophy had fallen into disuse. Thus this work has, from this combination of circumstances, been passed over; is seldom quoted except at second hand; and no edition of it, distinct from the other works of the author, or illustrated as the subject required, has appeared since that of Scaliger, published in 1619,—except one, accompanied by a French translation by Camus, in 1782, which is said to be incorrect, and is become scarce.

Dr Osborne proceeded to make a short analysis of the contents of this work, and shewed that Aristotle had anticipated Dr Jenner's researches respecting the cuckoo, as also some discoveries with respect to the incubated egg, which have been published within the last year. His observations on fish and cetaceous animals are curious in the extreme, as might be expected from the variety of these animals abounding in the Grecian seas. Those on insects it is difficult to appreciate, from uncertainty as to the names. He describes the economy of bees, as we have it at present; but mistakes the sex of the queen. He holds the doctrine of spontaneous generation in those cases in which he could not detect the ovary; an inevitable conclusion, arising from the want of the microscope, to which, and the want of knowledge of pneumatic chemistry, his principal errors are to be referred. The various organs are described as modified throughout the different classes of animals, (beginning with Man, the Βουλευτικον μονον), in nearly the same order as that afterwards adopted by Cuvier.

As specimens of the interesting matter treated of in the work, Dr Osborne selected the animal nature of sponges; the ages of various animals; the movements of the nautilus (the same doubt existing in the author's mind as to the origin of the shell, which has divided the opinions of Messrs Blainville, Owen, Gray, and Mad. Power, within the last year); the localities of animals, as affording data for ascertaining the rate at which they have extended themselves over the globe; particulars relating to artificial incubation as practised in Egypt; the management of cattle; a mode of fattening hogs with rapidity, by commencing with a fast of three days; the mohair goat located in Cilicia, as at present; hybernation and migrations of various animals and fish; description of the fisher-fish (*Lophius piscatorius*), and of the torpedo, with the proof that they catch their prey in the extraordinary manner described; many ingenious modes of taking partridge, and of fishing, detailed; the friendships which have been perpetuated between different classes of animals—as the trochilus and the crocodile, the *Pinna muricata* and the *Cancer pinnotheres*, the crow and the heron; their animosities, as between the crow and owl; the diseases of animals traced throughout the series, extending even to fish; hydrophobia described, as being communicated by the bite of a rabid dog to all animals except man, which appears to be the correct statement with respect to hot climates, and not (as has been represented by some modern travellers) an entire absence of the disease.

These detached specimens of the contents of this work, furnish, however, a very inadequate idea of its real value. There are in it whole sections, the separate sentences of which would furnish texts for as many Bridgewater Treatises. The freshness and originality of the observations taken from Nature herself, and not made up from quotations of preceding writers; the extent of the views, not bounded by any necessity for complying with preconceived or prevalent notions, but capacious as the author's mind itself, and frequently leading the reader into the most interesting under-currents of thought branching off from the great fountain; these are all merits belonging to the work, but not constituting its chief value,—which is, that it is a collection of facts, observed under peculiar advantages, such as have never since occurred, and *that it is at the present day to be consulted for new discoveries.*

Now that Greece is, for the first time since the revival of letters, in possession of a government capable of appreciating scientific investigations, a favourable opportunity offers for preparing an edition of the work, at once worthy of the age in which it was composed, and of that in which we live; and perhaps some individual may be found possessing a competent knowledge of the Greek language, and of zoology and comparative anatomy, who, after a sufficient examination of the animals now in Greece, shall undertake the task of editing and illustrating this great work. Such a a performance, properly executed, would be the resuscitation of a body of knowledge which has lain buried for above 2000 years, and would certainly be no less acceptable to zoologists and anatomists than to the cultivators of classical learning.—*Report of a paper read before the Royal Irish Academy on the 11th May last.*

15. *Migrations and Capture of the Rein-deer in North Siberia.*—The two most important epochs of the year, says Admiral Wrangell, are the spring and autumn migrations of the rein-deer. About the end of May they leave the forests, where they had some degree of shelter from the winter cold, in large herds, and seek the northern plains nearer the sea, partly for the sake of the better pasture afforded by the *moss tundras* (great deserts without any vegetation but moss and lichen), and partly to fly from the musquitoes and other insects, which, literally speaking, torment them to death.

The hunting at this season is not nearly so important and valuable as in the autumn; as it often happens that the rivers are still frozen over, they afford no opportunity of intercepting the deer, and the hunters can only lie in wait for them among the ravines, to shoot them with guns or arrows. Success with the latter weapon is rather uncertain, and the high price of powder and ball is an objection to the use of guns; the more so as at this season the rein-deer are very thin, and so injured by insects that nothing but the extremity of hunger can render the flesh palatable; the animals killed in spring are commonly only used for the dogs. The true harvest, which we arrived just in time to see, is in August or September, when the rein-deer are returning from the plains to the forests. They are then healthy and well fed, the venison is excellent, and, as they have just acquired their winter coats, the fur is thick and warm. The difference of the quality of the skins at the two seasons is such, that whilst an autumn skin is valued at five or six roubles, a spring one will only fetch one, or one and a half rouble.

In good years the migrating body of rein-deer consists of many thousands; and though they are divided into herds of two or three hundred each, yet the herds keep so near together as to form only one immense mass, which is sometimes from fifty to a hundred wersts in breadth. They always follow the same route, and in crossing the river near Plotbischtsche they choose a place where a dry valley leads down to the stream on one side, and a flat sandy shore facilitates their landing on the other side. As each separate herd approaches the river, the deer draw more closely together, and the largest and strongest takes the lead. He advances, closely followed by a few of the others, with head erect, and apparently intent on examining the locality. When he has satisfied himself, he enters the river, the rest of the herd crowd after him, and in a few minutes the surface is covered with them. Then the hunters, who had been concealed to leeward, rush in their light canoes from their hiding places, surround the deer, and delay their passage, whilst two or three chosen men, armed with short spears, dash into the middle of the herd, and dispatch large numbers in an incredibly short time, or at least wound them so that, if they reach the bank, it is only to fall into the hands of the women and children.

The office of the spearman is a very dangerous one. It is no easy thing to keep the light boat afloat among the dense crowd of the swimming deer, which, moreover, make considerable resistance; the males with their horns, teeth, and hind legs, whilst the females try to overset the boat by getting their fore feet over the gunnel; if they succeed in this the hunter is lost, for it is hardly possible that he should extricate himself from the throng; but the skill of these people is so great that accidents very rarely occur. A good hunter may kill one hundred or more in less than half an hour. When the herd is large, and gets into disorder, it often happens that their antlers become entangled with each other; they are then unable to defend themselves, and the business is much easier. Meanwhile the rest of the boats pick up the slain, and fasten them together with thongs, and every one is allowed to keep what he lays hold of in this manner. It might seem that, in this way, nothing would be left to requite the spearmen for their skill, and the danger they have encountered; but, whilst every thing taken in the rear is the property of whoever secures it, the wounded animals which reach the bank before they fall belong to the spearman who wounded them. The skill and experience of these men is such, that, in the thickest of the conflict, when every energy is taxed to the uttermost, and their life is every moment at stake, they have sufficient presence of mind to contrive to measure the force of their blows so as to kill the smallest animals outright, but only to wound the larger and finer ones, so as they may be just able to reach the bank. Such proceeding is not sanctioned by the general voice, but it seems nevertheless to be almost always practised.

The whole scene is of a most singular and curious character, and quite indescribable. The throng of thousands of swimming reindeer, the sound produced by the striking together of their antlers, swift canoes dashing in amongst them, the terror of the frightened animals, the danger of the huntsmen, the shouts of warning, advice, or applause from their friends, the blood-stained water, and all the accompanying circumstances, form a whole which no one can picture to himself without having witnessed the scene.

When the chase is over, and the spoils are distributed, the deer which have been killed are sunk in the river, the ice-cold water of which preserves them for several days, till there is time to prepare them for winter use. For this purpose the flesh is either dried in the air, smoked, or, if early frosts set in, frozen. The Russians sometimes salt the best pieces; the tongues are considered the greatest delicacy, and are reserved for special occasions.

Again, at page 203–4 of his Narrative, Admiral Wrangell gives the following account:—

The migratory rein-deer had not yet passed the river Aniuj at this place. Their arrival was expected with the utmost anxiety, for scarcity was already severely felt. It is not easy to imagine the fearful excess which famine reaches among a people whose support depends on one precarious incident. It often happens that many among them have to subsist during the latter part of summer almost entirely on the skins which form their bedding and clothing; and if happily a single rein-deer is killed, it is immediately cut up, divided among the whole tribe, and literally eaten, skin and all, the hair being just singed off. The contents of the stomach, and even the horns are used as food. Fish are not caught till later in the year, and even then only in small numbers; and few of the inhabitants venture to go off to the tundras in quest of game, for fear of missing the passage of the rein-deer, on which their support so essentially depends.

On the 12th of September, the hungry people were filled with joy by immense numbers of rein-deer approaching the right bank of the river opposite to Lobasnoji. I never saw such a multitude of these animals. At a distance their antlers resembled a moving forest. Crowds of people flocked in on every side, and hope beamed on every countenance as they arranged themselves in their light boats to await the passage of the deer. But whether the animals had seen and were terrified at the crowds of people, or whatever the reason may have been, after a short pause, they turned, left the bank, and disappeared among the mountains. The utter despair of the poor starving people was dreadful to witness. It manifested itself among these rude children of nature under various forms. Some wept aloud and wrung their hands; some threw themselves on the ground, and tore up the snow; others, and amongst them the more aged, stood silent and motionless, gazing with fixed and tearless eyes in the direction where their hopes had vanished. Feeling our utter inability to offer any alleviation to their misery, we hastened to quit this scene of woe, and resumed our voyage on the 30th.—*Wrangell's Narrative of an Expedition to the Polar Seas in the years* 1820-1-2-3. 8vo. Pp. 413. Madden & Co. London 1840.

16. *The Flying Squid or Cuttle Fish (Loligo Sp.)*—Many different kinds of Loligo are called by sailors Flying Squid, from a habit they have of leaping from the water, and proceeding through the air to some distance in a horizontal direction, like the flying fish. One kind of Loligo, or Flying Squid, which we captured in the Pacific Ocean, in Lat. 34° N., measured six inches in its entire length. The upper surface of the body is grey, freckled with purple, the under white; iris silvery, pupil jet black and prominent. It has eight arms and two tentacles. Each arm is furnished with a double row of suckers on its entire length; and all, with exception of the first or dorsal pair, have a loose membrane floating from their posterior surface. The two tentacles are round, slender, and twice the length of the arms, and have at their extremity a broad sickle-shaped membrane, covered with two rows of yellow hooks of different sizes.

This individual leaped from the sea over the high bulwarks of the ship and alighted on the deck, at a time when vast flocks of the same species were seen leaping around and often striking with violence against the bows of the vessel, the sea being comparatively smooth. The creature was much injured by the violence with which it had struck the deck, and shewed little animation; it did not attempt to leap or swim when put into a bucket of sea water, though it emitted a quantity of inky fluid* through a canal in the body, opening by a large orifice immediately below the neck. The prehensile power of the suckers on the arms was retained for a considerable time after the death of the animal; from which I should judge that, like the buckler of the sucking fish, their function in a great measure depends upon solely mechanical causes.

A second species, which we also obtained in the Pacific, resembled the above in size and form, but its two long tentacles, furnished at their extremities with rows of suckers (acetabula), instead of horny hooked appendages. The prevailing colours of this species are silver-white and steel-blue, spread with red spots and tints of violet and purple, a brilliant and very beautiful spot of emerald-green being placed immediately above each eye. We noticed examples of this family of Cephalopodes, from the equator to Lats. 34° N. and 16° S., Pacific Ocean.—*Bennet's Whaling Voyage*, vol. ii. p. 290.

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* This secretion is contained in a narrow oblong bag, of silvery hue, and placed immediately below the stomach.

17. *The Pilot Fish* (*Gasterosteus ductor*, Linn.).—The average length of this fish is about six inches, though we took one example in the Pacific which measured one foot two inches. The body is somewhat cylindrical, and neatly formed. The colour of the head and back is steel-blue; abdomen silvery; sides marked with five broad black bands; fins mottled black and white, and mostly tipped with white; inner circle of the iris hazel, outer gold-yellow. A single row of teeth in each jaw. Three short spines in front of the dorsal fin, and two in front of the anal. The lateral line is oblique, and terminates posteriorly in a semi-cartilaginous ridge, projecting from either side of the tail. The female is oviparous.

Pilot-fish are almost invariably found in attendance upon the shark, though the nature of their connexion with that ferocious fish is somewhat mysterious. They will accompany ships for a considerable time after their patron shark has been destroyed; but I am not aware that they have ever been, like the Remora, attending upon other large fish, whales, or miscellaneous floating bodies. The structure of their mouth, and the contents of their stomach, which are usually small fish, denote that they are accustomed to seek their food in a very independent manner. We captured many of them also by hook and line, baited with flesh; nor did they refuse the bait even when they were in company with a shark.

The reputation this fish has obtained of being the shark's pilot or provider (and which has sanctioned its trivial name), would appear to be groundless, were we guided only by the want of similar precedents in the animal kingdom. A fact, however, which came under my notice during a voyage from India, in the year 1832, led me to believe that there is some just foundation for this popular opinion. While we were becalmed in the Atlantic Ocean, a shark was seen close to the ship, and attended by two pilot-fish, which generally swam one above the other below him, and occasionally went off to some distance, as if to explore the surrounding sea; although it was seldom long before they returned, and resumed their former positions; the shark in the mean time, by its unwieldy form, slow movements, and lethargic aspect, offering a strong contrast to the sprightliness of his scouts. A baited hook was lowered from the bow of the ship; but the shark, when alone, passed it several times without notice, and apparently without seeing it. One of the foraging pilot-fish then approached the bait, and immediately swam off to where the shark was headed in a contrary direction; when the monster instantly turned, and followed his informant, which now swam a-head of him, in a direct line towards the suspended bait. He did not then hesitate a moment, but seized it, and was captured. While the shark was being hauled on board, the pilot-fish expressed the greatest concern, almost leaping out of the water in their endeavours to follow him, and swimming near the surface with every demonstration of anxiety. These faithful little fish were observed to attach themselves to the ship, but attracted little attention until some weeks afterwards, when we spoke the Thomas Grenville, East Indiaman, and lowered a boat to communicate with her. One of the fish was then seen to accompany the boat to and from the stranger ship, and so devotedly did it attend upon what it might have believed to be its lost shark, as to lead the officers of the Thomas Grenville to remark that we had a pilot fish *painted* on the rudder of the boat.

Their attendance upon sharks is somewhat capricious; we have seen more than five associated with one shark, while many others of the latter tribe, and assembled in the water at the same time, have not been accompanied by one of these fishes. They have evidently nothing to dread from the voracious companion they select, but swim around, and often a few inches a-head of him, as either their convenience or caprice may dictate.—*Bennet's Whaling Voyage*, vol. ii. p. 274.

18. *The Luminous Shark* (*Squalus fulgens*, N. Sp.)—There are so few well authenticated instances of a phosphorescent power existing in fish, as an attribute of life, that it is with some surprise we find this peculiarity inherent in the shark, a family so generally well known and described. The squalus which I have to notice is, however, a nondescript species, and one that certainly possesses a luminous power in a very high degree.

Two examples of this fish were accidentally taken, at different periods of the voyage, by a net towing on the surface of the sea. The first was obtained in Lat. 2½° S., Long. 163° W., and was ten inches in length. It was captured in the day-time, and, consequently, although its novel appearance attracted my attention, its phosphorescent power was not then noticed. The second specimen was taken at night, in Lat. 55° N., Long. 110° W. Its entire length was one and a half foot. Both fishes were alive when taken on board. They fought fiercely with their jaws, and had torn the net in several places. When the larger specimen, taken at night, was removed into a dark apartment, it afforded a very extraordinary spectacle. The entire inferior surface of the body and head emitted a vivid and greenish phosphorescent gleam, imparting to the creature, by its own light, a truly ghastly and terrific appearance. The luminous effect was constant, and not perceptibly increased by agitation or friction. I thought at one time that it shone brighter when the fish struggled, but I was not satisfied that such was the fact. When the shark expired, (which was not until it had been out of the water more than three hours), the luminous appearance faded entirely from the abdomen, and more gradually from other parts; lingering the longest around the jaws and on the fins. The only part of the under surface of the animal which was free from luminosity was the black collar around the throat; and while the inferior surface of the pectoral, anal, and caudal fins shone with splendour, their superior surface (including the upper lobe of the tail-fin) was in darkness, as also were the dorsal fins, and the back and summit of the head. I am inclined to believe, that the luminous power of this shark resides in a peculiar secretion from the skin. It was my first impression that the fish had accidentally contracted some phosphorescent matter from the sea, or from the net in which it was captured; but the most rigid investigation did not confirm this suspicion, while the uniformity with which the luminous gleam occupied certain portions of the body and fins, its permanence during life, and decline and cessation upon the approach and occurrence of death, did not leave a doubt in my mind but that it was a vital principle, essential to the economy of the animal.

The small size of the fins would appear to denote that this fish is not active in swimming; and since it is only predaceous, and evidently of nocturnal habits, we may perhaps indulge the hypothesis, that the phosphorescent power it possesses is of use to attract its prey, upon the same principle as the Polynesian islanders and others employ torches in night fishing. — *Bennet's Voyage*, vol. ii. p. 255.

19. *The "Trochilus and Crocodile" of Herodotus.*—Mr Wilkinson, in his excellent work on Egypt, vol. iii. p. 79, says: "Herodotus enters into a detail of the habits of the crocodile, and relates the frequent repeated story of the *trochilus* entering the animal's mouth during its sleep on the sand banks of the Nile, and relieving it of the leeches which adhere to its throat. The truth of this assertion is seriously impugned, when we recollect that leeches do not abound in the Nile; and the polite understanding supposed to exist between the crocodile and the bird, becomes more improbable when we examine the manner in which the throat of the animal is formed; for having no tongue, nature has given it the means of *closing it entirely*, except when in the act of swallowing, and,

during sleep, the throat is constantly shut though the mouth is open." Now, on this passage, says Mr W. C. Hurry, I have to observe, *first*, that I have seen many crocodiles caught, but very few that had not many leeches adhering to the inside of their mouths, and that these animals also infest the *argeelah*, and other animals which feed in the Ganges. *Secondly*, These leeches are not the *Hirudo medicinalis*, which Mr Wilkinson is probably correct in asserting not to be common in the Nile, as that species is usually found in running streams. The leech in question probably belongs to the genus Pontobdella, one species of which infests cod, skate, and other fish on the coasts of England. I have no doubt these creatures will be found as abundant in the Nile as they are in the waters of Bengal. *Thirdly*, Herodotus says, "the trochilus entering the crocodile's mouth devours the leeches." The crocodile is not said by Herodotus to be sleeping during the operation, as Mr Wilkinson asserts, otherwise the observation "that, pleased with the service, he never injures the trochilus," would be absurd. *Fourthly*, As to the polite understanding which Mr Wilkinson presumes, I may remark, that I believe the common *paddy bird* of Bengal to be the trochilus of Herodotus, or a bird of the same genus. Now, both Europeans and Bengalees agree in asserting, that this bird is constantly seen standing on the head of the crocodile; and though I never heard any one assert that he saw it in the act of picking his teeth for him, I think it will be admitted that the visit is not without an object.—*W. C. Hurry, Journal of Asiatic Society of Bengal for July* 1839.

ARTS.

20. *The Indian Mode of preparing the Perfumed Oils of Jasmine and Bela.*—Dr Jackson of Ghazeepore, in a letter to the editors of the Asiatic Journal of Calcutta for June 1839, says:—In my last communication on the subject of rose-water, I informed you that the natives here were in the habit of extracting the scent from some of the highly-smelling flowers, such as the jasmine, &c., and that I would procure you a sample, and give you some account of the manner in which it is obtained.* By the present steamer, I have dispatched two small phials, containing some of the oil procured from the Jasmine and the Bela flower. For this purpose the natives never make use of distillation, but extract the essence by causing it to be absorbed by some of the purest oleaginous seeds, and then expressing these in a common mill, when the oil given out has all the scent of the flower which has been made use of. The plan adopted is to place on the ground a layer of the flower, about four inches

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* *Vide* Edin. New Phil. Journ. vol. xxviii. p. 326, for account of cultivation of Roses, &c.

thick and two feet square; over this they put some of the Tel or Sesamum seed wetted, about two inches thick and two feet square; on this again is placed another layer of flowers, about four inches thick, as in the first instance; the whole is then covered with a sheet, which is held down by weights at the ends and sides. In this state it is allowed to remain from twelve to eighteen hours; after this the flowers are removed, and other layers placed in the same way; this also is a third time repeated, if it is desired to have the scent very strong. After the last process, the seeds are taken in their swollen state and placed in a mill; the oil is then expressed, and possesses most fully the scent of the flower. The oil is kept in prepared skins, called dubbers, and is sold at so much per seer. The Jasmine and Bela (Jasminum zamba) are the two flowers from which the natives in this district chiefly produce their scented oil; the Chumbul (Jasminum grandiflorum) is another, butI have been unable to procure any of this. The season for manufacture is coming on. The present oils were manufactured a year ago, and do not possess the powerful scent of that which has been recently prepared. Distillation is never made use of for this purpose, as it is with the roses, for the extreme heat (from its being in the middle of the rains when the trees come into flower) would most likely carry off all the scent. The Jasmine, or *Chymbele*, as it is called, is used very largely amongst the women, the hair of the head and the body being daily smeared with some of it. The specimen I send you costs at the rate of two rupees per seer.

21. *Preservation of Timber.*—M. Bouchirie has lately presented to the Royal Academy of Paris a memoir on the preservation of timber, in which he describes a process which he has invented, and which promises to be highly important. We have no space for details, but now remark that the process consists in imbuing, by means of absorption, the tissue of the wood with the pyrolignite of iron, immediately after the tree is felled, or even while still standing. This very simple operation is, it would appear, possessed of very extraordinary efficiency, 1*st*, In protecting timber against dry and humid rot; 2*d*, In augmenting its hardness; 3*d*, In preserving and increasing its flexibility and elasticity; 4*th*, In rendering its warpings, and consequently the fissures and disjointings which are so often witnessed after it has been wrought, nearly impossible, though freely exposed to great atmospheric changes of temperature; 5*th*, In greatly reducing its inflammability and combustibility; 6*th*, In conferring upon it colours and odours which are various and persistent. M. Bouchirie exhibited to the Academy a variety of specimens prepared according to his method, which were intrusted to a committee for examination.

NEW PUBLICATIONS.

1. *Sketch of the Geology of North America.* By Charles Daubeny, M.D., F.R.S., Professor of Botany and Chemistry in the University of Oxford.

This sketch, the perusal of which has afforded us both instruction and pleasure, we recommend to the attention of geologists.

2. *Supplement to the Introduction to the Atomic Theory, comprehending a sketch of certain opinions and discoveries bearing upon the General Principles of Chemical Philosophy, which have been brought into notice since the publication of that work.* By C. Daubeny, M.D., Professor of Chemistry and Botany. 8vo, pp. 62. Oxford. Murray, London, and Parker, Oxford. 1840.

Dr Daubeny's " Supplement to the Introduction to the Atomic Theory," like that work itself, noticed in a former volume of our journal, will particularly interest those who cultivate this important branch of Chemical Philosophy.

3. *A Manual of the Land and Fresh-Water Shells of the British Islands, with Figures of each of the kinds.* By Wm. Turton, M.D. A new edition, revised and enlarged by J. G. Gray, F.R.S. London, Longman & Co. 8vo, pp. 310.

Mr Gray has made nearly a new work of this by his numerous additions and corrections. It is the best guide for this department of the British Fauna.

4. *The Genera of Birds, with an Indication of the Typical Species of each Genus.* By G. R. Gray, Ornithological Assistant British Museum. London, R. & J. G. Taylor. 8vo, pp. 80.

This little volume will aid the young ornithologist and collector in threading their way through the never-ending mazes of Ornithological systems of arrangement.

5. *An Introduction to the Modern Classification of Insects, founded on the Natural Habits, and corresponding Organisation of the different Families.* By J. O. Westwood, F.L.S. &c. &c. London, Longman & Company. 2 vols. 8vo.

Mr Westwood's valuable, important, and learned work, by this time in the library of every lover of entomology, commences with interesting general observations on insects, and then proceeds to divide them into *orders;* each *order* is then taken up separately, and divided into *families.* The characters, habits, transformations, and general distribution of the insects comprised in each family are given with great accuracy, and in a very interesting manner; as are also the characteristic anatomical details, and preparatory states. The numerous figures with which these volumes are illustrated, which add so pre-eminently to its value, are, in almost every instance, original, and drawn by the author himself. Mr Westwood, with great modesty, says, "I have endeavoured to make my work a fitting "Sequel" to the "*Introduction to Entomology* of Messrs Kirby and Spence."

6. *Journal of the Asiatic Society of Bengal.* Edited by the Acting Secretaries. Year 1839.

Number for June contains—1. *Narrative of an Expedition into the Naga territory of Assam.* By E. R. Grange, Esq.—2. Report by Lieut. J. Glasfurd, Engineer, *on the progress made up to the* 1*st May* 1839, *on opening the Experimental Copper Mines of Kumaon.*—3. *Account of a Journey from Sumbulpur to Mednipur, through the forests of Orissa.* By Lieut. M. Kittoe (continued).—4. *Mr Middleton on the Meteors of August* 10. 1839.—5. *On the mode of preparing the Perfumed Oils of Jasmine and Bela.* By Dr Jackson.—6. *Report on the Assam Tea Plantations.* By C. A. Bruce, Esq.

Number for July.—1. *On the Bora Chung, or the Ground Fish of Bootan.* By J. T. Pearson, Esq.—2. *On the Gale and Hurricane in the Bay of Bengal, on the* 3*d*, 4*th, and* 5*th of June* 1839, *being a first Memoir in reference to the Theory of the Law of Storms in India.* By H. Piddington, Esq.—3. *Note on the "Trochilus and Crocodile" of Herodotus.* By W. C. Hurry, Esq.—4. *Documents relative to the application of Camel-Draught to Carriages;* communicated by C. B. Greenlaw, Esq.—5. Continuation of Lieut. M. *Kittoe's Journey through the Forests of Orissa.*—6. *Meteorological Register.*

Number for August.—1. *Part* 2*d of Researches on the Gale and Hurricane in the Bay of Bengal on the* 3*d*, 4*th, and* 5*th June* 1839. By H. Piddington, Esq.—2. *Extracts from Mr M. M'Clelland's paper on the India Cyprinidæ.*—3. *On the Smelting of the Iron-Ore of the District of Burdwan.*—4. *On the habits of the Coel, and on the Discovery of Isinglass.* By Major Davidson.—5. *Note on the Scapes of Xanthorhæa and Fossil Stems of Lepidodendra.* By Lieut. N. Vicary.—6. *Proceedings of the Asiatic Society.*—7. *Meteorological Register.*

List of Patents granted in Scotland from 18*th March to* 18*th June* 1840.

1. To Joseph Schofield of Littleborough, in the county of Lancaster, cotton-spinner and fustian manufacturer, and Edmund Leach of Littleborough, aforesaid, manager of cotton-spinners and weavers, for an inven-

tion "of certain improvements in looms for weaving various kinds of cloth."—18th March 1840.

2. To William Maltby junior, of Mile-End, chemist, and Richard Cuerton, brassfounder, of Percy Street, in the county of Middlesex, for an invention of "improvements in extracting and concentrating the colour, tanin, and other matters contained in vegetable and animal substances."—18th March 1840.

3. To Sir William Burnett of Somerset House, in the county of Middlesex, Knight Commander of the Royal Hanoverian Guelphic Order, for an invention of "improvements in preserving animal, vegetable, woollen, and other fibrous substances, from decay."—25th March 1840.

4. To Peter Lomax of Little Bolton, in the county of Lancaster, weaver, for an invention of "certain improvements in looms for weaving."—26th March 1840.

5. To Peter Bancroft of Liverpool, in the county of Lancaster, merchant, and John MacInnes of the same place, manufacturing chemist, for an invention of "an improved method of renovating or restoring animal charcoal, after it has been used in certain processes or manufactures, to which charcoal is now generally applied, and thereby recovering the properties of such animal charcoal, and rendering it again fit for similar uses."—6th April 1840.

6. To William Hunt of the Portugal Hotel, Fleet Street, in the city of London, manufacturing chemist, for an invention of "improvements in the manufacture of potash and soda, and their carbonates."—11th April 1840.

7. To Thomas Robinson Williams of Cheapside, in the city of London, gentleman, for an invention of "certain improvements in the manufacture of woollen and other fabric or fabrics, of which wool or fur form a principal component part, and in the machinery employed for effecting that object."—11th April 1840.

8. To Henry Philip Rouquette of Norfolk Street, Strand, in the county of Middlesex, merchant, for an invention, communicated by a foreigner residing abroad, for an invention, "being a new pigment."—18th April 1840.

9. To Pierre Auguste Ducôte of No. 70 Saint Martin's Lane, in the county of Middlesex, lithographer, for an invention of "certain improvements in printing china, porcelain, earthenware, and other like wares; and for printing on paper, calicoes, silks, woollens, oilcloths, leather, and other fabrics; and for an improved material to be used in printing."—20th April 1840.

10. To William Stone of Winsley, in the county of Wilts, gentleman, for an invention of "improvements in the manufacture of wine."—20th April 1840.

11. To John Inkson of Ryder Street, St James's, in the county of Middlesex, gentleman, for an invention, communicated by a foreigner residing abroad, of "improvements in apparatus for consuming gas for the purposes of light."—20th April 1840.

12. To Jean Francois Victor Fabien of King William Street, in the city of London, gentleman, for an invention, communicated by a foreigner residing abroad, of "improvements in rotary engines to be worked by steam or other fluid."—20th April 1840.

13. To Matthew Uzielli of King William Street, in the city of London, merchant, for an invention, communicated by a foreigner residing

abroad, of "certain improvements in the arrangement and construction of ships' hearths, or apparatus for cooking, and for obtaining distilled or pure water from salt or impure water."—22d April 1840.

14. To THOMAS AITKEN of Chadderton, in the county of Lancaster, manufacturer, for an invention of "certain improvements in the machinery or apparatus for drawing cotton and other fibrous substances."—22d April 1840.

15. To ANGIER MARCH PERKINS of Great Coram Street, in the county of Middlesex, civil-engineer, for an invention of "improvements in apparatus for transmitting heat by circulating water."—6th May 1840.

16. To ORLANDO JONES of the City Road, in the county of Middlesex accountant, for an invention of "improvements in treating or operating on farinaceous matters to obtain starch and other products, and in manufacturing starch."—6th May 1840.

17. To FRANCIS GYBBON SPILSBURY of Wallsall, Staffordshire, chemist, MARIE FRANCOIS CATHARINE DOETZER CORBAUX of Upper Morton Street, in the county of Middlesex, and ALEXANDER SAMUEL BYRNE of Montague Square, in the county of Middlesex, gentleman, for an invention of "improvements in paints or pigments, and vehicles, and in modes of applying paints, pigments, and vehicles."—7th May 1840.

18. To JOSEPH CLINTON ROBERTSON of 166 Fleet Street, in the city of London, patent agent, for an invention, communicated by a foreigner residing abroad, for "an improved method or methods of obtaining mechanical power from electro-magnetism, and the engine or engines by which the said power may be made applicable to motive purposes."—7th May 1840.

19. To JOHN WILSON of Liverpool, in the county of Lancaster, lecturer on chemistry, for an invention of "an improvement or improvements in the process or processes of manufacturing the carbonate of soda."—11th May 1840.

20. To ANTOINE BLANC of Paris, in the kingdom of France, merchant, and THEOPHILE GERVAIS BAZILLE of Rouen, in the same kingdom, merchant, now residing at Sablioniere's Hotel, Leicester Square, in the county of Middlesex, for an invention, communicated by a foreigner residing abroad, of "certain improvements in the manufacturing or producing soda and other articles obtained by or from the decomposition of common salt, or chloride of sodium."—11th May 1840.

21. To ROBERT GILL RANSON of Ipswich, paper-maker, and SAMUEL MILLBOURN of the same place, foreman to the said Robert Gill Ranson, for an invention of "improvements in the manufacture of paper."—13th May 1840.

22. To JAMES KNOWLES of Little Bolton, in the county of Lancaster, coal-merchant, for an invention of "an improved arrangement of apparatus for regulating the supply of water to steam-boilers."—13th May 1840.

23. To THOMAS MYERSCOUGH of Little Bolton, in the county of Lancaster, manager, and WILLIAM SYKES of Manchester, in the county of Lancaster, machine-maker, for an invention of "certain improvements in the construction of looms for weaving or producing a new or improved manufacture of fabric, and also in the arrangement of machinery to produce other descriptions of woven goods or fabrics."—13th May 1840.

24. To HENRY TREWHITT of Newcastle-on-Tyne, in the county of Northumberland, Esq., for an invention of "certain improvements in the fabrication of china or earthenware, and in the machinery or apparatus applicable thereto."—15th May 1840.

25. To William Winsor of Rathbone Place, in the county of Middlesex, artists' colourman, for an invention of "a certain method or certain methods, process or processes, of preparing, preserving, and using colours."—15th May 1840.

26. To William Craig of Glasgow, in the kingdom of Scotland, engineer, and William Douglas Sharp of Stanley, Perthshire, in the same kingdom, engineer, for an invention of "certain improvements in machinery for preparing, spinning, and doubling cotton, flax, wool, and other fibrous substances."—18th May 1840.

27. To Alexander Angus Croll of Greenwich, in the county of Kent, manufacturing chemist, for an invention of "certain improvements in the process of manufacturing gas, and in the production of ammoniacal salts."—19th May 1840.

28. To John Davidson, salt-manufacturer, Leith Walk, near Edinburgh, in the county of Edinburgh, for an invention of "an improvement in the method of preserving salt."—19th May 1840.

29. To Thomas Walker of Galashiels, in the county of Selkirk, mechanic, for an invention of "improvements in apparatus applicable to feeding machinery employed in carding, scribbling, or teazing fibrous materials."—26th May 18 0.

30. To James Hadden Young of Lille, in the kingdom of France, at present residing at 32 Norfolk Street, Strand, in the county of Middlesex, merchant, and Adrian Delcambre of Lille aforesaid, manufacturer, for an invention of "an improved mode of setting up printing types."—28th May 1840.

31. To John Hawley of Frith Street, Soho Square, in the county of Middlesex, watchmaker, for an invention, communicated by a foreigner residing abroad, of "improvements in pianos and harps."—29th May 1840.

32. To Thomas Edmondson of Manchester, in the county of Lancaster, clerk, for an invention of "certain improvements in printing presses."—1st June 1840.

33. To William Potts of Birmingham, in the county of Warwick, brass-founder, for an invention of "certain apparatus for suspending and moving pictures and curtains."—2d June 1840.

34. To Elijah Galloway of Water Lane, Tower Street, in the city of London, engineer, for an invention of "improvements in steam-engines."—9th June 1840.

35. To Francois Vouillon of Prince's Street, Hanover Square, in the county of Middlesex, silk-mercer, for an invention, communicated by a foreigner residing abroad, of "improvements in the manufacture of ornamental woven fabrics."—9th June 1840.

36. To William Daubney Holmes of Cannon Row, in the city of Westminster, and county of Middlesex, civil engineer, for an invention of "certain improvements in the construction of iron ships, boats, and other vessels, and also in means for preventing the same from foundering, also in the application of the same improvements, or parts thereof, to other vessels."—18th June 1840.

37. To John Crighton junior of Manchester, in the county of Lancaster, machine-maker, for an invention of "certain improvements in machinery for weaving single, double, or treble cloths, by hand or power."—18th June 1840.

THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

On the Diminution of Temperature with Height in the Atmosphere, at different seasons of the year. By JAMES D. FORBES, Esq. F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*

IN the year 1830, I succeeded in establishing a Register of the Thermometer at the Bonally Reservoir, which formerly supplied the city water-works, being at a distance of five miles in a direction south-west from Edinburgh. This station is on the northern acclivity of the Pentland Hills, at a height of 1100 feet above the sea. The following year I obtained corresponding observations at the village of Colinton, situated a mile and a half north of the preceding station, and above 700 feet lower. Although this difference of level be not very considerable, yet, as these comparative registers have been kept for nearly five years with pretty uniform results, some confidence is evidently due to the conclusions, even although considerable difficulties opposed themselves to obtaining registers quite free from exception. The interest attaching to them is the greater, that, although registers have been kept at Leadhills and other elevated stations, I do not recollect any strictly comparative observations in Scotland, perhaps not even in Great Britain, at two stations near one another, and differing considerably in level, from which the important meteorological element of the decrement of temperature in the atmosphere could be deduced.

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* From Transactions of the Royal Society of Edinburgh, vol. xiv.; read 1st April 1839.

The Bonally station is situated on the exposed northern acclivity of the Pentland Hills, without any kind of shelter. Its elevation above the mean level of the sea was very accurately determined by myself trigonometrically, and the thermometer hung a height of precisely 1100 feet. The exposure was the north side of a cottage, which has since been allowed to fall to ruin. The observations were made by Mr Johnston, the officer appointed by the Water Company for the inspection of their works, and by his family. I have every reason to believe that they were made and registered with perfect fidelity, although, from want of practice, they may have been occasionally erroneously entered. They were made daily at 8 A.M. and 8 P.M. The thermometer was a mercurial one, now in my possession, which, by comparison with a standard one, I find reads pretty constantly 0°.35 too high.* The readings have therefore been diminished by that quantity.

The Colinton station was at the School-house there, and the observations were carefully made and registered by my friend the Rev. R. Hunter. The height of the thermometer above the mean level of the sea, ascertained by myself, is 364 feet. The hours of observation were the same as above. The thermometer has been carefully compared with a standard, and the error in different parts of the scale not being uniform, it has been ascertained, and a corresponding correction applied.

By far the greater part of the calculation of these observations was performed by my late friend and pupil Mr John Spens, son of Dr Thomas Spens, who, had he lived, must ultimately have distinguished himself in a profession which rarely fails to reward real talent. Much of the remaining calculation was kindly undertaken by Mr John T. Harrison.

The mean temperature of each month at each station at 8 A.M. and 8 P.M. being taken, the mean difference for each month of the year for the whole period is deduced, and hence the mean for the entire period, which gives a decrement for 736 feet of ascent, amounting to 3°.27 for the morning observations, 3°.18 for the evening, or 3°.22 for both, which corresponds to 229 feet of ascent for 1° of decrement of temperature.

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* For one year only a spirit thermometer was employed.

This decrement is rather rapid, and is, no doubt, partly to be accounted for by the comparatively sheltered situation of the lower station.

The influence of the season of the year on the decrement of temperature is particularly striking, as the following Table shews; and that the discrepancies it contains are not generally errors of observation, is pretty clear, from the agreement of the morning and evening columns, and various other tests, which it is not necessary to mention.

TABLE I.

Calculation of the Mean Temperature of each Month during the Years 1831–32–33–34–35, *at Bonally and Colinton, and corrected for the errors of Graduation of Thermometers.*

DATE.	8 A. M.					8 P. M.				
	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.
Jan. 1832	35.61	39.29	35.26	38.97	3.71	35.93	38.39	35.58	38.06	2.48
— 1833	31.58	31.77	31.23	31.23	0.00	32.64	33.77	32.29	33.32	1.03
— 1834	37.74	39.32	37.39	39.00	1.61	38.06	40.16	37.71	39.86	2.15
— 1835	33.97	36.39	33.62	36.00	2.38	34.71	38.16	34.36	37.82	3.46
			137.50	145.20	7.70			139.94	149.06	9.12
		Mean	34.38	36.30	1.92		Mean	34.98	37.26	2.28
Feb. 1831	34.93	37.11	34.58	36.75	2.17	35.75	37.64	35.40	37.29	1.89
— 1832	36.31	39.00	35.96	38.68	2.72	37.00	38.45	36.65	38.12	1.47
— 1833	35.14	37.86	34.79	37.50	2.71	36.36	38.61	36.01	38.28	2.27
— 1834	36.43	37.46	36.08	37.10	1.02	37.46	39.50	37.11	39.19	2.08
— 1835	36.89	39.78	36.54	39.48	2.94	36.43	40.21	36.08	39.92	3.84
			177.95	189.51	11.56			181.25	192.80	11.55
		Mean	35.59	37.90	2.31		Mean	36.25	38.56	2.31
Mar. 1831	37.61	42.42	37.26	42.24	4.98	38.45	43.00	38.10	42.82	4.72
— 1832	36.68	42.48	36.33	42.28	5.95	37.68	40.64	37.33	40.37	3.04
— 1833	34.87	38.48	34.52	38.15	3.63	35.39	37.48	35.04	37.13	2.09
— 1834	39.16	42.61	38.81	42.40	3.59	37.93	41.84	37.58	41.60	4.02
— 1835	36.26	38.35	35.91	38.02	2.11	36.48	38.42	36.13	38.10	1.97
			182.83	203.09	20.26			184.18	200.02	15.84
		Mean	36.56	40.61	4.05		Mean	36.83	40.00	3.17
Apr. 1831	41.30	45.47	40.95	45.38	4.43	42.34	45.10	41.99	45.00	3.01
— 1832	42.13	46.63	41.78	46.56	4.78	41.13	44.63	40.78	44.50	3.72
— 1833	41.37	44.67	41.02	44.55	3.53	39.90	43.83	39.55	43.68	4.13
— 1834	41.37	46.53	41.02	46.46	5.44	40.43	46.23	40.08	46.15	6.07
— 1835	40.97	45.60	40.62	45.51	4.89	40.00	45.80	39.65	45.72	6.07
			205.39	228.46	23.07			202.05	225.05	23.00
		Mean	41.08	45.69	4.61		Mean	40.41	45.01	4.00

TABLE I.—continued.

DATE.	8 A. M.					8 P. M.				
	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.
May 1831	46.48	50.39	46.13	50.40	4.27	46.58	49.93	46.23	49.93	3.70
— 1832	45.48	50.93	45.13	50.95	5.82	45.19	49.32	44.84	49.31	4.47
— 1833	52.06	56.97	51.71	57.20	5.49	51.74	55.19	51.39	55.40	4.01
— 1834	49.41	53.97	49.06	54.10	5.04	49.90	53.80	49.55	53.95	4.40
— 1835	45.61	50.10	45.26	50.10	4.84	45.39	49.87	45.04	49.87	4.83
			237.29	262.75	25.46			237.05	258.46	21.41
		Mean	47.46	52.55	5.09		Mean	47.41	51.69	4.28
June 1831	55.23	58.97	54.88	59.24	4.36	55.77	58.27	55.42	58.52	3.10
— 1832	53.97	57.83	53.62	58.09	4.47	53.07	57.00	52.72	57.24	4.52
— 1833	53.50	57.37	53.15	57.62	4.47	52.30	55.90	51.95	56.12	4.17
— 1834	54.43	57.57	54.08	57.81	3.73	54.97	56.97	54.62	57.20	2.58
— 1835	53.37	55.43	53.02	55.64	2.62	51.90	55.27	51.55	55.48	3.93
			268.75	288.40	19.65			266.26	284.56	18.30
		Mean	53.75	57.68	3.93		Mean	53.25	56.91	3.66
July 1831	57.68	60,06	57.33	60.36	3.03	58.27	60.77	57.92	61.07	3.15
— 1832	55.16	59.03	54.81	59.30	4.49	55.52	57.84	55.17	58.09	2.92
— 1833	57.03	59.61	56.68	59.90	3.22	55.68	58.55	55.33	58.82	3.49
— 1834	56.16	59.19	55.81	59.47	3.66	57.00	59.26	56.65	59.54	2.89
— 1835	55.77	57.64	55.42	57.90	2.48	54.81	57.07	54.46	57.31	2.85
			280.05	296.93	16.88			279.53	294.83	15.30
		Mean	56.01	59.38	3.37		Mean	55.90	58.96	3.06
Aug. 1831	57.08	59.71	56.71	60.00	3.29	55.90	59.55	55.55	59.84	4.29
— 1832	55.23	57.52	54.88	57.77	2.89	53.68	55.48	53.33	55.70	2.37
— 1833	52.06	55.55	51.71	55.76	4.05	50.45	54.97	50.10	55.16	5.06
— 1834	55.84	59.87	55.49	60.17	4.68	54.48	59.19	54.13	59.47	5.34
— 1835	57.58	58.84	57.23	59.12	1.89	56.00	58.97	55.65	59.25	3.60
			276.02	292.82	16.80			268.76	289.42	20.66
		Mean	55.20	58.56	3.36		Mean	53.75	57.88	4.13
Sept. 1831	50.87	54.27	50.52	54.44	3.92	50.97	54.14	50.62	54.30	3.68
— 1832	51.77	53.87	51.42	54.02	2.60	50.63	53.40	50.28	53.54	3.26
— 1833	49.13	52.83	48.78	52.95	4.17	48.57	52.47	48.22	52.57	4.35
— 1834	50.83	52.70	50.48	52.80	2.32	50.07	52.87	49.72	52.98	3.26
— 1835	50.03	50.80	49.68	50.83	1.15	49.17	51.27	48.82	51.32	2.50
			250.88	265.04	14.16			247.66	264.71	17.05
		Mean	50.18	53.01	2.83		Mean	49.53	52.94	3.41
Oct. 1831	49.71	53.10	49.36	53.22	3.86	50.06	52.13	49.71	52.21	2.50
— 1832	47.03	50.77	46.68	50.80	4.12	46.45	49.16	46.10	49.14	3.04
— 1833	45.61	48.06	45.26	48.03	2.77	45.32	47.45	44.97	47.40	2.43
— 1834	45.58	48.87	45.23	48.85	3.62	45.52	48.87	45.17	48.85	3.68
— 1835	42.51	44.77	42.16	44.66	2.50	41.87	44.45	41.52	44.33	2.81
			228.69	245.56	16.87			227.17	241.93	14.46
		Mean	45.74	49.11	3.37		Mean	45.49	48.38	2.89

TABLE I.—continued.

DATE.	8 A. M.					8 P. M.				
	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.	Bonally.	Colinton.	Bonally corrected.	Colinton corrected.	Difference.
Nov. 1831	36.73	40.03	36.38	39.73	3.35	37.73	39.50	37.38	39.19	1.81
— 1832	38.20	39.40	37.85	39.09	1.24	38.67	40.10	38.32	39.80	1.48
— 1833	38.17	40.67	37.82	40.39	2.57	38.63	39.70	38.28	39.40	1.12
— 1834	40.30	41.37	39.95	41.13	1.18	40.23	42.53	39.88	42.33	2.45
			152.00	160.84	8.34			153.86	160.72	6.86
		Mean	38.00	40.08	2.08		Mean	38.46	40.18	1.72
Dec. 1831	39.23	41.81	38.88	41.57	2.69	38.93	41.81	38.58	41.58	3.00
— 1832	36.19	39.61	35.84	39.30	3.46	36.81	39.93	36 46	39.63	3.17
— 1833	35.87	38.42	35.52	38.09	2.57	36.10	38.53	35.75	38.20	2.45
— 1834	39.35	39.77	39.00	39.47	0.47	39.35	41.35	39.00	41.10	2.10
			149.24	158.43	9.19			149.79	160.51	10.72
		Mean	37.31	39.61	2.30		Mean	37.45	40.13	2.68

TABLE II.—*General Synopsis.*

MONTH.	8 A. M.			8 P. M.			Mean Diff.	Feet of Ascent for 1° Fahr.
	Bonally.	Colinton.	Diff.	Bonally.	Colinton.	Diff.		
January .	34.38	36.30	1.92	34.98	37.26	2.28	2.10	351
February .	35.59	37.90	2.31	36.25	38.56	2.31	2.31	319
March . .	36.56	40.61	4.05	36.83	40.00	3.17	3.61	204
April . .	41.08	45.69	4.61	40.41	45.01	4.60	4.60	159
May . . .	47.46	52.55	5.09	47.41	51.69	4.28	4.69	157
June . . .	53.75	57.68	3.93	53.25	56.91	3.66	3.79	194
July . . .	56.01	59.38	3.37	55.90	58.96	3.06	3.22	229
August . .	55.20	58.56	3.36	53.75	57.88	4.13	3.74	197
September	50.18	53.01	2.83	49.53	52.94	3.41	3.12	236
October .	45.74	49.11	3.37	45.49	48.38	2.89	3.13	235
November .	38.00	40.08	2.08	38.46	40.18	1.72	1.90	387
December .	37.31	39.61	2.30	37.45	40.13	2.68	2.49	296
	531.26	570.48	39.22	529.71	567.90	38.19	38.70	
General Mean	44.27	47.54	3.27	44.14	47.32	3.18	3.22	229

I have compared the results of this Table, which are projected in the uppermost curve of Plate II., with the results obtained on the far larger scale of a difference of level of 6836 English feet, between Geneva and the Convent of St Bernard, as given by Kaemtz in the second volume of his *Lehrbuch der Meteorologie.* Considering the different circumstances of the two, these curves (which I have purposely reduced to a similar range) approximate wonderfully. They both indicate a most rapid increase of the Difference of Temperature between February and March, and a most rapid decline in November, the maximum being about May.

That the decrement of temperature with height is most rapid in summer, and least in winter, has been long known;[*] but I am not aware of any attempt to account for the law of its variation at different seasons. The following considerations will probably be found satisfactory.

If we examine the annual curves of mean temperature at Colinton and Bonally, projected in the lower part of Plate II., we shall find that they differ in three respects, whilst there is a remarkable coincidence in their general features. (1.) The entire Bonally curve stands lower on the paper than the Colinton curve, because the mean temperature of any and every part of the year is lower. (2.) It is a *flatter* curve than the Colinton curve, or the range of the thermometer is less; consequently the *minima* differ *less* than if the two curves had been similar, and the *maxima* differ *more*. This is the reason why the decrement of temperature with height is most rapid in summer, and least so in winter. (3.) Not only is the Bonally curve lower than the Colinton one, and flatter, but it is *shifted* to the right hand, so that the maxima occur later, as well as the minima and mean temperatures. A little attention will likewise shew that a gap or vacuity must be left between the curves, greatest whilst the temperature rises, and least whilst it falls; and also that the difference of the vertical ordinates of the two curves will be greatest when they form the greatest ascending angle with the horizontal axis, and least when the descending angle is greatest, that is, as inspection shews, in May and November respectively, which agrees with the results of the uppermost curve of the plate.

The examination of these curves furnishes us with some data of the most important kind for meteorology, which it is best in the first place to state, and afterwards to consider how we can explain.

The first fact is the familiar one, that mean temperature diminishes as we ascend in the atmosphere. The second is, that the annual range diminishes as we rise, and, at a certain height, would probably sensibly vanish. The third is, that the influence of seasons begins to be felt at the plains, and is later communicated to the mountains. The two former of these facts obtain with reference to the diurnal as well as annual variation of temperature; the last appears to be in that case reversed.*

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[*] See my Report on Meteorology in the first volume of the British Association Reports.

The *shift* of the annual curve, or retardation of epochs, and likewise the decreased range, is common to the strata of the air above the surface of the earth, and to those of the soil beneath it. Both ultimately, no doubt, exhibit a limit, first where the diurnal variations disappear, then the annual. The cause, however, is very different in the two cases, the one being chiefly the result of the radiation and the other of the conduction of heat.

It is only curious that the diurnal curve seems to follow so different a law, at least in summer;—perhaps the reason is, that the direct solar radiation is more energetic in that case, and the vehicular conveyance of heat by the air (or convection) less. Thus, with respect to the process of annual heating, the earth's surface (considered as an extensive plain) is the point where the sun's rays freely transmitted by the atmosphere first become productive of any considerable warmth. That warmth is propagated slowly and progressively by conduction to the inferior strata of earth, and by convection to the superior strata of air; in either case, as I have said, a *later* and a *feebler* impress of the annual curve is found. The diurnal temperature is probably much more modified by the *direct* effects of radiation. The detached mountain tops, more exposed and less massive, receive and part with the solar heat more rapidly than the low country, presenting a complete analogy, the former with an insular, the later with a continental climate. The summits change temperature rapidly, the extremes are less; but the changes of the heat of the plain follow later, and are more marked. This is not conjecture; many facts might be quoted to support it, but the following is sufficient, that

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* Saussure, Voyages dans les Alpes, tom. iv. § 2050, &c. See also Kaemtz, Lehrbuch, band ii. s. 133.

Saussure, in the part of his work already cited, finds, that, whilst the minimum temperature occurred at 4 A. M. (in the month of July) both at the Col du Géant and at Geneva, the former station had acquired the mean temperature of the day at 6 A. M., which at Geneva occurred three hours later; and, during the decline of temperature in the afternoon, the mean recurred at the Col du Géant from half an hour to an hour *sooner* than at Geneva.

There are other causes besides those just mentioned which contribute to distinguish the daily from the annual curve. Of these the more important are the more gradual character of the annual change of temperature, and the influence of humidity. The former affects our experiments by preventing the ascending and descending currents from being instantly established, in the manner that the law of specific gravity would assign; and when radiation is least intense (as in winter), and the moving power therefore small, this transfer is often impeded, and even the law of densities violated altogether. Thus we know to be often the case in winter and in cold climates, that the higher strata are the warmer. To place this in a clear point of view, I shall add a table shewing the number of times in each month that this has occurred, which is indicated under the column headed "Number of times negative :" considering the differences of temperature simultaneously observed at Bonally and Colinton as *positive* when the former (the higher station) was colder than the latter; and *vice versa*. I have also added the extreme positive and negative values for each month; and though here, more than any where else, errors of observation and registration are likely to have crept in, yet we cannot but be struck with the number of times in which the common law of density has been reversed, and the great excess of warmth observable at the higher station on some occasions, especially in autumn and winter. I would repeat, however, that the observation of these extremes is less likely to be invariably correct than any other part of the tables. Dividing the year into four seasons, the following summary, which includes both morning and evening observations, is, from the extent of the induction, entitled to considerable confidence.

	Mean Difference.	Times out of 100 negative.	Mean of greatest + values in each Month.	Mean of greatest — values in each Month.
SPRING (March, April, May)	4°.30	7.1 per cent.	10°.5	2°.87
SUMMER (June, July, August)	3°.58	13.5	10°.3	4°.8
AUTUMN (Sept., October, Nov.)	2°.72	15.0	9°.8	7°.4
WINTER (Dec., January, Feb.)	2°.30	15.6	9°.8	5°.9

These numbers have been obtained from the following Table, which contains the details.

TABLE III.

	8 A. M.				8 P. M.			
	No. of observations.	No. of times negative in each Month.	Greatest + value.	Greatest — value.	No. of observations.	No. of times negative in each month.	Greatest + value.	Greatest — value.
Jan. 1832	31	1	16	— 1	31	4	13	— 2
—— 1833	31	9	4	— 7	31	5	5	— 7
—— 1834	31	7	13	— 9	31	12	10	— 7
—— 1835	31	2	10	— 7	31	3	16	— 5
Feb. 1831	28	4	8	— 7	28	5	9	— 8
—— 1832	29	3	9	— 7	29	9	7	— 4
—— 1833	28	2	8	— 1	28	3	12	— 4
—— 1834	28	8	5	— 6	28	7	15	— 7
—— 1835	28	2	12	—10	28	2	12	— 6
Mar. 1831	31	1	14	— 2	31	1	14	— 2
—— 1832	31	1	10	— 1	31	2	8	— 6
—— 1833	31	1	7	— 1	31	1	6	— 2
—— 1834	31	3	12	— 6	31	2	17	— 7
—— 1835	31	7	6	— 5	31	6	7	— 2
Apr. 1831	30	2	8	— 2	30	2	6	— 3
—— 1832	30	3	9	— 2	30	3	13	— 3
—— 1833	30	3	7	— 1	30	0	9	0
—— 1834	30	0	14	— 0	30	0	15	0
—— 1835	30	2	13	— 3	30	2	17	— 2
May 1831	31	2	10	— 2	31	3	9	— 1
—— 1832	31	1	9	— 3	31	1	11	— 3
—— 1833	31	3	11	— 2	31	4	12	— 9
—— 1834	?~	~	~	~	~	~	~	~
—— 1835	31	3	10	— 5	31	2	10	— 4
June 1831	30	0	10	0	30	5	11	— 3
—— 1832	30	3	11	— 8	30	4	17	— 2
—— 1833	30	2	9	— 4	30	1	8	— 1
—— 1834	30	3	6	— 4	30	6	10	—12
—— 1835	30	4	9	—10	30	6	15	— 9
July 1831	31	7	9	— 2	30	5	10	— 3
—— 1832	31	2	12	— 2	31	5	10	— 3
—— 1833	31	3	8	— 5	31	4	8	— 3
—— 1834	31	2	6	— 4	31	5	9	—12
—— 1835	31	11	13	— 5	31	8	11	— 8
Aug. 1831	31	5	10	— 2	31	1	10	— 3
—— 1832	31	6	7	— 4	31	6	6	— 8
—— 1833	31	1	10	— 2	31	2	11	— 3
—— 1834	31	2	17	— 6	31	2	15	— 5
—— 1835	31	9	10	— 6	31	5	12	— 5
Sept. 1831	30	3	9	— 4	30	1	8	— 2
—— 1832	30	4	12	—13	30	4	18	— 5
—— 1833	30	2	15	— 2	30	1	11	— 2
—— 1834	30	5	6	— 6	30	6	9	— 8
—— 1835	30	11	7	— 7	30	9	11	— 6
Oct. 1831	31	0	7	——	31	2	4	— 6
—— 1832	31	2	15	— 5	31	3	13	— 2
—— 1833	31	5	10	— 4	31	5	12	— 7
—— 1834	31	4	14	— 7	31	5	17	— 7
—— 1835	31	3	5	—10	31	4	10	— 7
Nov. 1831	30	2	7	— 8	30	5	7	— 9
—— 1832	30	5	5	—10	30	5	5	—10
—— 1833	30	7	9	—19	30	8	12	—20
—— 1834	30	10	8	— 7	30	5	12	— 6
Dec. 1831	31	2	7	— 8	31	5	7	— 9
—— 1832	31	1	11	— 3	31	1	10	— 1
—— 1833	31	1	7	— 3	30	5	9	— 7
—— 1834	31	9	6	—13	31	10	13	— 5

The influence of humidity I believe to be very important in modifying the results. The distribution of moisture as we rise in the atmosphere varies extremely at different seasons. In spring the hills are chilled by continued condensations of moisture, whilst the plains are comparatively dry; and in autumn the reverse often occurs. I believe that the actual fall of rain on low and high grounds confirms this view, the autumnal rains being often heaviest in the plains, whilst in spring and summer the excess is amongst the hills.*

The curve in Plate II.,† representing the mean daily range for five years, is deduced from careful observations made at Edinburgh by Mr Adie, with self-registering instruments.‡

On the Comparative Illuminating and Heating power of different kinds of Coal-gas Burners, and on the use of Coal-gas as a source of Heat. By Andrew Fyfe, M. D., F. R. S. E. Communicated by the Society of Arts.

I. *On the Comparative Illuminating Power of Gas Burners.*§

It is well known that coal-gas varies in its composition and quality, according to the process followed in its manufacture. After being freed from impurities, it consists chiefly of hydrogen, carbonic oxide, and different varieties of hydro-carbons. The hydrogen, carbonic oxide, and light hydro-carbons, constitute by far the largest proportion; but it is the heavy hydro-carbons that are the true source of light, for though all of them are inflammable, yet the former give very little light during their combustion.

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* Professor Forbes in a postscript appended to this memoir, adds, "I am glad to find that the reasoning I have employed in page 211, to account for the *diurnal* variations of the decrease of heat in the atmosphere, is entirely confirmed by the observations of Eschmann, Kaemtz, and Horner in Switzerland, recorded in Poggendorf's *Annalen*, xxvii. 345, and in Dove's *Repertorium*, iii. 331." Edit.

† The vertical lines in the plate correspond to the middle of each month.

‡ The latter part of this paper has been remodelled since it was read.—*Dec.* 1839.

§ Read before the Society of Arts for Scotland 11th March 1840.

It may naturally be supposed, that if these gases are supplied with a due proportion of air for combustion, the amount of light should be always the same; and that, consequently, when the gas is completely burned, it is of little consequence in what sort of burner it is consumed. This is not, however, the case. No doubt, when the gas is completely burned by a due admission of air, we arrive at the greatest amount of light that is to be expected, but we may err in supplying air *too freely;* and though by this the whole of the elements are united with its oxygen, and carbonic acid and water are formed to the same extent as before, yet we may find that much less light is afforded. Hence, also, the reverse will occur, when the supply which has been too liberal is diminished; the light for the same consumpt of gas is then increased.

This must be accounted for on the principle established by Sir H. Davy, that a gas burns with a white light when it contains an ingredient, which is not volatilized by the heat generated during its combustion. The element of this nature in coal-gas is carbon, which, though in union with hydrogen, may be set free, and *in its uncombined state* be then ignited and burned, in which case it will afford white light. Unless, therefore, the gas be so consumed that it will undergo decomposition before it is actually burned, it will not afford much light; and it is chiefly this which causes the difference in the illuminating power of burners differently constructed.

From what has been said, it is evident that coal-gas may be burned in two ways. In the one, it may be burned strictly as a hydro-carbon; by which, both the hydrogen and carbon will at once, and as if simultaneously, unite with oxygen; in the other way, it may, during the combustion, be so far decomposed, that the hydrogen will first combine with oxygen, and set the carbon free, and in this free and uncombined state it will then unite with oxygen: now, in the former case, it will burn and not give much light, in the latter the light will become intense; and hence the necessity of attending to this in consuming coal-gas for the purposes of illumination.

That what has been said with regard to the decomposition of gas really occurs, is shewn by setting fire to it, and holding a piece of wire-gauze in the flame. When held in the white flame, carbon, in the form of smoke, instantly appears, but when put in the blue flame there is no smoke. Or if the gauze be held over the burner, and at a little distance from it, so as to allow the gas before it is kindled to mix with air, and a flame is then applied above the gauze, the combustion goes on also without smoke, but with very little light,—the ingredients at once entering into union with the oxygen of the air, without the carbon being previously set free from the hydrogen.

It is evident then, that when gas is to be used for affording light, it ought to be so consumed that there will be decomposition; that, in fact, it must not be burned as a hydro-carbon, by the ingredients at once and at the same instant entering into union with oxygen, but by the hydrogen first combining with oxygen, and setting the carbon free, which is then to unite with oxygen; and this is to be accomplished by the due admission of air. Hence it is, that not only do different burners afford very different light for equal consumpt of gas, but the same burners will, under different circumstances, also vary in their illuminating power.

The truth of these observations has been fully and satisfactorily illustrated, in an elaborate and interesting paper published by Drs Christison and Turner in the Edinburgh Philosophical Journal, 1825. While engaged in a series of experiments on the illuminating power of coal-gas and oil-gas, they were led to examine the circumstances affecting the emission of light,—such as the height of flame, the peculiar construction of burners, and the shape of the glass chimney; their experiments relating chiefly to argands. With regard to the length of flame, they found that the light was not always in proportion to the consumpt, even when the same burner was used. Thus taking a jet flame of three inches as the standard, and considering its light and consumpt as 100, the light afforded for equal expenditures was, for a flame of two inches, 100; three inches, 109; four inches, 131; five inches, 150; six inches, 150.

In these trials, by lengthening the flame, there was of course an increased consumption of gas, but then the light was increased in a greater ratio. When of five inches in length, the light was as 150 to 100; in other words, there was a gain of 50 per cent. Beyond this, there was no farther gain by the farther lengthening of the flame.

The same was found to occur with argands, and in a greater proportion. Thus, with a five-holed burner, varying from half an inch to five inches, the light for equal consumpt of gas was, for a flame of half an inch, 100; one inch, 282; two inches, 560; three inches, 582; four inches, 582; five inches, 604: so that, by raising the flame from half an inch to three inches, the light for the same quantity of gas burned was nearly six times greater. Beyond this length of flame there was little farther increase. In this last trial the standard was a four-inch jet, and, as before, the light and consumpt were taken as 100.

These experiments I have myself verified repeatedly, as will be afterwards noticed.

It may be considered, then, as a rule with regard to argands, that the higher the flame is made, without smoking or acquiring a dingy hue, the illuminating power becomes greater in proportion than the increase in consumpt occasioned by the lengthening, and this is to be accounted for on the principle already stated; for when the flame is low, too much air is admitted in proportion to the escape of gas, and consequently most of it is burned without being decomposed; whereas, when the flame is high, the air is in less proportion; decomposition is effected, and the carbon is separated from the hydrogen and then burned, and gives by its combustion in this way the white light, and thus adds to the illuminating power.

It is evident, however, that the flame cannot always be kept at its highest elevation; for as light draught or shaking of the lamp is apt to occasion smoke, or a sudden change in the flame, by which the chimney is apt to be broken; and this is more likely to occur in open apartments as in shops.

The different circumstances detailed in the paper alluded to, regarding the size of the apertures in argands, their proximity to one another, the width of the glass chimney, and its distance from the sides of the burner, have left little farther to be done with regard to the best means of consuming gas in this way. These experiments relate almost exclusively to argands. I have thought that it would be equally interesting to extend the investigation to other forms of burners, such as those with flat flames, and which are used without chimneys, so as to shew their illuminating power compared with jets and argands, and also the most economical way of using them. The chief object, then, which I have had in view in the experiments which I am now to relate, is to point out the illuminating power of different kinds of burners as compared with one another; and I have thought it necessary to make these preliminary observations, to shew the necessity of attending to the circumstances which affect the consumpt and combustion of the gas, by which the light afforded is so materially influenced.

In conducting these experiments, I have invariably used the coal-gas as supplied by the Gas Company. Though it varies occasionally a little in its illuminating power, yet as the experiments are merely comparative, and as the results of the trials made on the same day are contrasted with each other, this slight difference does not affect them; besides, it must be borne in mind, that it is not my wish to fix with numerical precision the amount of light afforded, but merely to ascertain the comparative degrees of light given by the different burners.

In all of the trials I have used a single jet burner as my standard; and to secure this giving forth always the same light, it was connected with a gasometer, so adjusted as to burn exactly one foot per hour, when the flame was five inches. The gasometer employed for this purpose was on the usual construction,—a copper vessel suspended in water, and counterpoised by weights attached to cords passing over pulleys. I am aware that it may be objected to this apparatus, that as the gas-holder sinks in the water it becomes of less and less weight, proportionally to the counterpoises, and that consequently the pressure is not always the same. I was of course aware of this, but I found on trial that, for short distances, the gas-holder moved, the difference in pressure was trifling, particularly when the gas from it was used when the gas-holder was not very much out of, or very much sunk in, the water. Accordingly, I found that, for equal times, the consumpt was the same, or so very nearly so that the difference was unworthy of notice, in the experiments of the nature of which I was engaged.

I have already mentioned, that, according to Christison and Turner, when the flame of a jet is gradually lengthened, the light becomes in a greater and greater ratio than the consumpt of gas, till it reaches five inches, beyond which there is no farther gain; and that this is the case, I have myself verified by numerous trials. We may consider, then, five inches as the most economical height for a single jet. Accordingly, in using it as my standard, I have invariably so employed it, that I might the more easily be enabled to compare the lights given by other burners with it, and with each other, for equal consumpts of gas.

The method to which I resorted for judging of the illuminating power is that of Rumford; the depth of shadow afforded by the flames; of course, attending to the circumstances noticed by him which are necessary to secure accuracy.

I am aware that many object to this method, as being in several respects fallacious; but these objections are of little force, when the instrument is used for ascertaining the comparative illuminating power of lights, such as gas flames, where the shadows do not vary much in their colour from each other. The standard jet was generally kept at a fixed distance; the other lights, after being brought to the proper height, were moved till the shadows were the same; and in doing this, I was assisted by others in whose accuracy I could rely. I mention this, to shew that I did not trust entirely to my own observation in judging of the shadows, and that reliance may the more readily be placed in the results detailed.

The gas used in the moveable burners was measured by a gas-meter; and to secure still farther accuracy in the results, a cubic foot passed through the metre was thrown into the gasometer connected with the standard jet, and accurately marked off on its scale. The pressure on the gas from the metre varied from 15 to 19–10ths of an inch of water; that on the standard gasometer was kept at 17.

In the paper by Drs Christison and Turner, and also in that published by Professor Brande in Phil. Trans. 1820, it is shewn, that, when an argand burner contains a few holes, and at such a distance that the flames from them do not meet, it may be considered as so many single jets; but when the holes are nearer, and the flames are united, then the combustion is carried on differently; hence the superior efficacy of the burners now used compared with those formerly recommended. By that now in general use, which has twenty-four holes, of about the 40th of an inch in diameter, the flame, however short, is one continued ring, and, when high, there is little of the blue flame, shewing that the gas is properly consumed, so as to afford a comparatively great amount of light.

One of these argands of twenty-four holes 1–40th of an inch, the diameter of the circle of holes being 7–8ths of an inch, and the diameter of the central air aperture 6–8ths of an inch, was compared with the standard jet of five inches; and burning one foot per hour. I found that, when the flame was only 1½ inch,

The consumpt was as .	1 jet to 3.4 argand
The light . . .	1 ... 2.65

With another of the same kind, the flame being 1¾ of an inch,

The consumpt was as . .	1 jet to 3 argand
The light . . .	1 ... 2.85

In both of these trials, there was a loss compared with the light given by the jet,—thus confirming the experiments of Christison and Turner already alluded to.*

With the same burner used in the last experiment, when the flame was raised to 2½ inches,

The consumpt was as . .	1 to 3.75
The light, ...	1 to 5.512

Now, as 3.75 : 5.512 :: 1 : 1.46; so that, in this case, there was actually a gain of 46 *per cent.* for equal consumpt of gas.

The same burner, with a 3-inch flame,

Consumpt, ...	1 to 4.137
Light, ...	1 to 7.441

And as 4.137 : 7.441 :: 1 : 1.79—Gain 79;

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* The area of the apertures recommended by Drs Christison and Turner is 1–32 of an inch, but about 1–40th is that now generally used in this town.

with a flame of 3½ inches,

Consumpt, ... 1 to 4.36
Light, ... 1 to 8
And 4.36 : 8 : : 1 : 1.83—Gain 83.

The width of the glass chimney was 17–10ths of an inch.

Another burner of a similar nature, but having more holes, was tried. The central aperture for air was 17–20ths of an inch in diameter; the diameter of the circle of holes 21–20ths; the number of holes 42 of the 50th of an inch in diameter. The flame in the first trial was 1 inch.

Consumpt, ... 1 jet to 2.85 argand
Light, ... 1 ... 2.145

so that there was a loss in this case as compared with the jet.

When burned with 1½-inch flame,

Consumpt was . . 1 to 3.15
Light, . . . 3.24 very little gain;

with flame of 2 inches,

Consumpt, ... 1 to 4
Light, ... 1 to 5.304
And as 4 : 5.304 : : 1 : 1.32—Gain 32.

with flame of 2¾ inches,

Consumpt, ... 1 to 4.8
Light, ... 1 to 7.035
And 4.8 : 7.035 : : 1 : 1.46—Gain 46;

with 3½-inch flame,

Consumpt, ... 1 to 5.45
Light, ... 1 to 9.94
And 5.45 : 9.94 : : 1 : 1.82—Gain 82.

This burner, then, having a greater number of holes, and consuming gas in the ratio of 5.45 per hour to the other as 4.36, did not yield a greater light for the same consumpt. It is remarkable, however, how very nearly the trials with these burners agree, when the gas is burned under favourable circumstances, and they shew, also, how much saving there is by the use of burners of the kind mentioned, the light of a jet and of these burners being, for equal consumpt of gas, in the ratio of 100 to about 182; consequently, to procure the same light, there is an immense saving when properly constructed argands are used.

Other argands were tried, but, though these were found to vary in their illuminating power according to the height of flame, yet they did not, when burned with the flame high, come up in their illuminating power to the burners already noticed.

A ten-holed argand, for instance, when burning with a two-inch flame, and consuming 2.85 feet per hour, did not give so much light, in proportion to the consumpt, as the jet; but when the flame was raised till the consumpt became 3.75 feet per hour, the

Light was as 1 jet to 4.9 argand
Consumpt, 1 ... 3.75
And as 3.75 : 4.9 : : 1 : 1.30—Giving a gain of 30.

The next kind of burner tried was the *bat-wing.* In this the gas escapes by a slit instead of minute apertures; of course the quantity of gas consumed must depend very much on the size of the slit. Those I employed were the bat-wings in common use, the slit being such as to admit a piece of watch-spring.

In the first trial the gas was burned in much smaller quantity than the burners would allow to escape, and compared with the standard jet of five inches as before.

In the first trial,

Consumpt, . 1 jet to 1.875 bat-wing
Light, . . 1 ... 2.626
And as 1.875 : 2.626 : : 1 : 1.40—Gain 40.

In the second trial,

Consumpt, . 1 to 2.307
Light, . . 1 to 3.403
And 2.307 : 3.403 : : 1 : 1.42—Gain 42.

In the third trial,

Consumpt, . 1 to 3.75
Light, . . 1 to 6.16
And 3.75 : 6.16 : : 1 : 1.64—Gain 64.

Another bat-wing, of rather smaller dimensions, was tried.

Consumpt, . 1 to 2.14
Light, . . 1 to 2.88
And as 2.14 : 2.88 : : 1 : 1.34—Gain 34.

Again,

Consumpt, . 3.53
Light, . . 5.618
And 3.53 : 5.618 : : 1 : 1.59—Gain 59.

In these trials the results so far agree with those obtained by argands, viz. that, when the flame is low, the gas is not so profitably consumed as when it is high; the air most probably being supplied too freely, by which the gas is not sufficiently decomposed before it is burned.

The results also shew, that, for equal consumpts, the bat-wings do not afford so much light as good argands. Taking the jet as 100, the argand gave a light as about 180; whereas the bat-wing did not exceed 164, for equal consumpts.

The next burners tried were fish-tails, or the decussating jets. In this burner, the apertures, of the same diameter as those in the single jets, pass in a slanting direction out from the tube of the burner, and as the gas rushes out, it is, as it were, twisted round, so that a line passing along the flame is diagonal to one passing in the direction of the holes.

The standard jet, as usual, burning one foot per hour. The fish-tail was made to burn so as to give the full flame, but without any flickering.

In the first trial,

Consumpt, . 1 jet to 2.3 fish-tail
Light, . . 1 ... 3.16
And 2.3 : 3.16 : : 1 : 1.37—Gain 37.

In another,

Consumpt, . 1 to 2.14
Light, . . 1 to 2.98
And 2.14 : 2.98 : : 1 : 1.39—Gain 39.

In a third trial,

Consumpt, . 2.18
Light, . . 3.16
And 2.18 : 3.16 : : 1 : 1.40—Gain 40.

Numerous other trials of a similar nature were made with *fish-tails*, from which I have found that the gain, by using this burner instead of the jet, is, on an average, about 40 per cent. It is of the utmost consequence, however, to be careful in keeping the apertures clean, more especially when the flame is burned low, as in bed-rooms, when it is wished during night merely to keep it burning. In this case the holes are apt to become clogged with a carbonaceous deposit, and the gas is then burned unprofitably; indeed this is the case with almost all burners.

Thus, in one case where a burner was tried, which had been long in use in a bed-room, the

Consumpt was as 1 jet to 2.14 fish-tail
Light, . . 1 ... 2.44
And 2.14 : 2.44 : : 1 : 1.14—Gain only 14.

Now, in this case, the consumpt of the gas was the same as in one of the former instances, but the light was very inferior; the former, when consuming 2.14, giving a gain over the jet of 39, whereas this, consuming the same, gained only 14.

Hence, then, the absolute necessity of keeping the burner clean; the easiest method of doing which is pushing occasionally a bristle into the holes; or, which is perhaps preferable, rubbing the surface of the burner with a hair-brush.

When, instead of using a single fish-tail, the flames from two fish-tails are brought together by their flat surfaces, the light is very much increased, as is at once evident by the increase of illumination in the apartment. It occurred to me that a burner constructed on this principle might be used with advantage, provided the increase of light is not attended with a proportional increase in the consumpt. On trial I did not find this to be the case. Thus, when two fish-tails were burned separately, the consumpt of gas was by both 4.44, compared to that of the standard jet as 1; and when burned with their flames united it was as 5, but the light was much increased: thus, compared with the jet, the

Consumpt was . 1 jet to 5 fish-tails
Light, ... 1 ... 8.192
And as 5 : 8.192 : : 1 : 1.63—Gain 63.

In another trial the gain on the jet was 59; thus making the average 61. Now, in the experiments with single fish-tails already given, the gain did not go beyond 40; so that by this double fish-tail burner there is an evident increase of light; but, though this occurs, the gain is not greater than when a good bat-wing, burning with its full allowance of gas, is used; besides the flame is by no means so steady.

Other kinds were also tried, with the view of improving the light, when small quantities of gas are required. The first of these was composed of jets formed by drilling holes in a straight line, at such a distance that the flames from them united. I had burners of this kind made with two, three, four, and five holes; but I did not find, when they were allowed to burn with a flame of the same height as the jet, that there was any increase in the light, over and above that from the increased consumption.

Thus, with a burner with three holes, of the same size as that in the single jet, and the flame about three inches, the

Consumpt was .	1 jet to 1.07 jets
Light, . . .	1 ... 1.10

so that the gain was a mere nothing.

With a four-inch flame it was the same.

With four holes and five holes, when the flames were from three to five inches, there was a loss of light compared with the jet; the increase in light not keeping pace with the increased consumpt.

When, however, these burners are made to give the same light as the jet of five-inch flame, there is a saving; for, when the single jet consumes gas as 100, these compound jets consume only as from 80 to 90; thus giving a saving for the same amount of light of from 10 to 20 per cent. They might, therefore, be used advantageously, but they are liable to one objection,—the flame is not so steady as that of the single jet, unless when used with a chimney.

Considering the superior efficacy of the bat-wing, I was induced to try a burner constructed on the same principle, but so made as to consume a much smaller quantity of gas; and I have found this to answer well. I have had them made of different sizes, but the chief object I had in view was to get one which would consume the same, or nearly the same, as a single jet. I did not, however, find, when this was done, that there was any saving. When, for instance, the slit is small, the flame is similar to that of a jet; but, when the slit is larger, then the flame becomes broader, and, though more gas is consumed, there is an increase in light beyond the increased consumpt.

Thus, with a burner having a small slit, when compared with the standard jet, in one instance, the consumpt was 1 jet to 0.97 slit; in another, 1 to 1.07; but the light was the same.

The slits were then enlarged a little, and the results were:

Consumpt, . . 1 to 1.76
Light, . . . 1 to 2.52
1.76 : 2.52 : : 1 : 1.43—Gain 43.

With another burner,

Consumpt, . . 1 to 1.66
Light, . . . 1 to 2.485
1.66 : 2.485 : : 1 : 1.49—Gain 49.

In a third,

Consumpt, . . 1 to 1.66
Light, . . . 1 to 2.55
1.66 : 2.55 : : 1 : 1.53—Gain 53.

In a fourth,

Consumpt, . . 1 to 1.46
Light, . . . 1 to 2.278
And 1.46 : 2.278 : : 1 : 1.56—Gain 56.

In a fifth,

Consumpt, . . 1 to 1.57
Light, . . . 1 to 2.5
1.57 : 2.5 : : 1 : 1.59—Gain 59.

So that, for equal consumpt of gas, there was a gain varying from 40 to 60 *per cent.*

I consider a burner of this kind as extremely useful, where no great degree of light is required. Not that I prefer it to the large bat-wing, or even to the fish-tail, though it gives more light than the latter for the same consumpt of gas; but for many purposes it may be found useful, as for street lamps, instead of jets.

In making these remarks on the comparative illuminating power of gas-burners, I wish it to be borne in mind, that it has not been my object to fix them with numerical precision. It must be evident, from what has been said, that the statements given must be considered merely as *general results*, for we must always recollect that the light afforded depends not only on the kind of burner, but, even with the same burner, on extraneous circumstances; as the height of flame, the freedom of the burner from carbonaceous deposit, the form of chimney when argands are used, and many others.

It is not my intention at present to enter upon the practical applications resulting from these experiments. I will refer to them after detailing the experiments on the heating powers of the gases. I may here merely remark, that, looking at the results of the trials now given, we may consider the argand as by far the most economical way of consuming gas when illumination is the only object in view; besides, the appearance of the burners naturally gives it a preference to others. As to the kind of argand, that now in general use, with 24 holes about one-fortieth of an inch, seems to answer the purpose better than any other hitherto recommended, but the area of the aperture must be increased or diminished a little according to the illuminating power of the gas. Of course, much must depend on the height of the flame and the kind of chimney. For general purposes, the flame ought to be such, that the gas will burn without smoke, and without the flame acquiring a dingy hue. This will be found to be about three inches in length. As to the chimney, it ought to be such as will supply the air in due proportion, but not in excess, otherwise the gas is not decomposed previous to its combustion, and consequently, is burned unprofitably, but for particulars on these points, I must refer to the paper by Christison and Turner.*

For other purposes, the bat-wing or the fish-tail ought to be preferred, as where it is not convenient to use chimneys, as in shops and manufactories. As to the single jet burner, it is evidently the most unprofitable way in which gas is consumed. Where the light required is not great, instead of the single jet, a burner with three apertures on a line and near each other, ought to be employed, because with these to get the same light as with a jet a smaller quantity of gas is requisite.

II.—*On the Comparative Heating Power of Gas-Burners.*†

I have already mentioned that when gas is used for the purpose of illumination, it is not merely necessary that it be perfectly consumed; the supply of air must also be properly regulated, otherwise the gas does not undergo that degree of decomposition which is necessary for the separation of the carbon from the hydrogen, by the combustion of the former of which white light is produced. A question therefore naturally arose at the outset of the investigation on the heating power. Is this subject to the same law? or, when the gas is completely consumed, is the heat evolved the same, whether there is or is not previous decomposition? I conceived it necessary to establish this first, because if it were found that the heating power is not subject to the same law, the subsequent steps of the investigation would be much facilitated.

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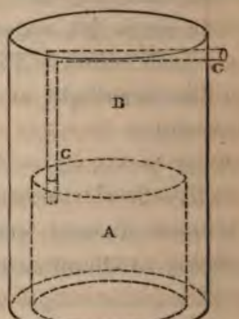
* Since the experiments related in this paper were performed, a paper has been published in the last number of the Journal, by Sir John Robison, giving the results of his trials with argands of different kinds. To this communication, the reader is referred for valuable information on the construction of burners, and on the comparative amount of light afforded by them. The general results will be found to agree with those above recorded.

† Read before the Society of Arts for Scotland, 13th May 1840.

In the experiments on the heating power, the gas was measured by passing it through the same meter that was used for the illuminating power. To ascertain the amount of heat evolved, the method which most naturally occurred, was merely to use the same quantity of water in the same vessel, always at the same temperature, and to find the degree to which it would be raised by the consumpt of equal quantities of gas. This would not, of course, give the actual amount of heat, but as the object in view was merely to ascertain the comparative heating power, it was considered sufficiently correct. The apparatus which I used was merely a boiler, B, with its sides extended farther down than the bottom, and hollow, so as to form a cavity A, into which the flame was placed; CC is a tube to carry off the products of combustion. This was preferred to a common pot or flask, because the flame being in a great measure surrounded by water, the heat that would otherwise be lost by radiation was absorbed; besides, the heated products were transmitted by the tube C through the water, so as to give off the greater part of their heat. In using this apparatus, when the flame of the gas is placed too far up in the cavity, owing to the products of combustion not being carried off quickly, there is frequently a good deal of smoke; it is ne-



cessary, therefore, to attend to this, so as to secure the complete combustion of the gas, which is done by bringing the burner a little lower. In the comparative trials, the same quantity of water at the temperature 45, was put into the boiler, which was always covered with a lid. Of course the flame of the gas was at different distances from the bottom of the boiler, according to the kind of burner used, and according to the length of the flame.

The first experiments were made with jets, fish-tails, batwings, and argands, burning under most favourable circumstances for affording light. For equal consumpts of gas, the experiments on the illuminating power have shewn, that, taking the jet as the standard, and giving light as 100, the fish-tails give light as 140, the batwings as about 160, and the argands as about 180. In each experiment, a quarter of a gallon of water at 45° was put into the boiler, and half a foot of gas was consumed by each burner. The temperature of the room varied from 58° to 62°.

Burners.	Time required.	Temp.	Degrees gained.
Jet, . . .	30 min.	140	95
Fish-tail, . .	13 ...	146	101
Bat-wing, . .	9 ...	142	97
Argand 10 holes, .	12 ...	142	97
... 24, . .	9 ...	142	97
... 42, . .	7 ...	144	99

The results of these experiments do not exactly coincide, but as the flames of some of the burners were more within the cavity of the boiler than others were, there may have been a slight loss of heat by radiation. Supposing this to be the case, it would appear that the heating power is not affected by the same circumstance as the illuminating power; that, in other words, the heat afforded by different kinds of gas-burners is not in proportion to the light they afford, but to the quantity of gas they consume. Whether or not this was the case, was to be proved by farther trials with the burners used in different ways, so as to make them consume different quantities of gas in the same time, or to require different times for the same consumpt. In the following trials the gas used was half a foot.

Burners.	Flame.	Time.	Temp.	Gain.
Jets, . .	3 inches	40 min.	147	102
Fish-tail, .	half full	40 ...	132	87
Bat-wing, .	quarter full	30 ...	134	89
Argand, .	1 inch	15 ...	142	97

In the above, the results with the jet and argand nearly agree with the former. Those with the fish-tail and bat-wing, though they coincide with themselves, yet do not agree with the others. In the two first, the whole of the gas was consumed; in the two others, there was occasionally a little smoke, which may account for the deficiency.

The following trials were made with the 42-holed argand, at various heights of flame, a quarter of a foot of gas being used.

Flame.	Time.	Temp.	Gain.
3 inches, with chimney,	4 min.	100	55
1 inch, do.	12 ...	100	55
1 inch, none,	12 ...	100	55
½ inch, none,	15 ...	100	55
Just visible, none, All blue.	30 ...	90	45

In all of these, with the exception of the last, the heat evolved was exactly proportionate to the consumpt of gas. In the last, we may account for the deficiency by the time required, in consequence of which part of the heat that the water had gained must have been given off to the surrounding air.

From the experiments that have been given, there can, I think, be no doubt with regard to the conclusion to be drawn,—that the heating power of the different kinds of burners now in use, is just in proportion to the quantity of gas that they consume, provided of course the combustion is perfect; in other words, it is of no consequence whether the gas is consumed with previous decomposition so as to give much light, or with a bluish flame affording little light; the heat is always the same for equal consumpts. Experiments to be afterwards mentioned, will still farther prove the truth of this. Allowing that this is the case, it is evident that, when gas is used solely for affording light, and when at the same time it is of consequence to avoid as much as possible its heating effect, argands ought to be employed, because, for equal consumpts, they give much more light than other kinds of burners; consequently, to get the same light, a smaller quantity of gas is requisite, and the heating effect will be proportionally less. On the contrary, when, along with light, it is desirable also to have as much heat as possible, then fish-tails should be used, because, to give the same light as the argands, a larger quantity of gas must be consumed.

III. *On the use of Gas as a source of Heat.*

From what has been said, it appears that, for equal quantities of gas consumed by the different kinds of burners now in use, the same amount of heat is evolved, and that, therefore, it is of no consequence how the gas is burned, provided the combustion is perfect; but in using gas as a source of heat, other methods have been recommended, and different means have been resorted to for economizing it; and from the due application of which, we may be enabled to answer the important question, whether gas may be employed with economy for this purpose?

For many purposes, the argand burner with a copper chimney will be found a very convenient method of applying heat. It has, however, this objection, that, unless used with caution, it is apt to smoke. The object to be warmed ought never to be brought too near the top of the chimney.

For most purposes, a much more convenient form of burner is the rose jet, which is merely a tube terminated by a hollow flat top, of an inch or two inches in diameter, according to circumstances, and the sides of which are perforated with apertures, at the distance of about a quarter of an inch from each other. By this means we have a series of jets, burning nearly horizontally, so that the flame may be spread out to a greater or less extent, as it may be required. The only precaution necessary, is to take care that the apertures are at such a distance from one another, that the flames shall not rush together when they are burned low, and by which smoke is occasioned.

This burner I found by numerous trials, like those previously mentioned, gives heat just in proportion to the consumpt of gas, whether burning with the flames large or small.

There is still another method of using gas as a source of heat. It is that recommended by Sir John Robison, and described in the paper alluded to. For this purpose, a piece of fine wire-gauze is placed on a tube of about two or three inches in diameter, and of about thirty inches in length. The tube, open below, is put over the pipe from which the gas issues which, as it escapes, mixes with the air in the tube, and passing along with it through the gauze, is then consumed, the air for the combustion being supplied up through the tube.

From the results of the trials with the burners already mentioned, there was every reason for believing, that the heat afforded would be the same when the gas was consumed on gauze, but as the general impression is the reverse, it was necessary to put it to the test of experiment.

In the different trials, I made use of gauze of various diameters, on tubes of different lengths, and also consumed the gas, sometimes with the flame barely visible in day light, at other times with more or less admixture of white. The water in the boiler, as before, was a quarter of a gallon, at temperature 45. With gauze of three inches diameter, on a tube of twenty inches in length, the gas burning with a blue flame a little way up in the cavity of the boiler, half a foot, in fifteen minutes, water rose to 145.

In another trial, the flame on the gauze being larger, the same quantity of water was raised to 151. In numerous other trials, the temperature varied a little, the elevation ranging from 145 to 150. The discordance in the results may be satisfactorily accounted for from the manner in which the flame was placed. When large, it cannot be all within the cavity of the boiler; of course there is a considerable loss, and there is also frequently an escape of part of the gas unconsumed, either owing to its not being thoroughly mixed with air, or owing to the products of combustion not being carried off with sufficient rapidity, by which the perfect combustion is prevented. Hence the difficulty of getting accurate results when the gas is burned in this way. We may, however, from the experiments, draw the conclusion that the heat evolved when the gas is consumed on gauze, is the same as when it is burned in any other way; and, consequently, when used as a source of heat, it is, generally speaking, of little consequence how it is consumed, in so far as regards the heat evolved; the evolution depending, not on the *manner* in which the gas is burned, but on the *quantity*, provided of course that the combustion is complete; and it is of the utmost consequence to attend to this, for when any of it escapes combustion, not only is there a loss of heat, but there is also an offensive smell, more especially when gauze is used.

With respect to the method of combustion to be preferred, much must depend on the object to be heated. When it is small, as a glass flask or a pot, and it is wished to heat it quickly, I prefer the argand with a copper chimney; for larger objects, the rose jets or wire-gauze may be used. The former of these may be of different sizes, according to circumstances, but the apertures must always be at such a distance from each other, that, when the gas is inflamed, the flames shall not run together, so as to cause smoke. In using these burners, the nearer the flames are to the object to be heated the better, but they must not come in contact with it, because then there is smoke. With regard to the gauze, air must be freely supplied from below, and the gauze ought not to be near the object, otherwise the gas is not all consumed.

Though gas, when consumed by these means, gives out heat in proportion to the consumption, much must depend on the *form of vessel* employed, as, in one case, more of the heat may be successfully applied than in another. When, for instance, the burner, whether a common burner or gauze, is placed below a common pot or a glass flask, much of the heat is lost by radiation, and also by the current of warm air, which, though striking the sides of the vessel, is carried off without having time to communicate its heat to it. Hence, if we employ a vessel so formed as to surround the flame, much of the heat that would otherwise be lost is saved. Thus, when a 24-holed argand was placed under a pot, with the chimney half an inch from the bottom of the pot, by half a foot of gas consumed in seven minutes, water was raised from 45 to 80, giving a gain of 35. When the same flame was put under the boiler already described, the same quantity of water was raised to 110, giving a gain of 65. With other burners the results were similar, the heat communicated to the boiler being nearly double of that acquired by the common pot.

There is another circumstance of material consequence to be attended to. When gas is burned, watery vapour is formed by the union of the oxygen of the air with the hydrogen in the gas. As long as this is kept warm by the products of combustion, it is retained in the aeriform state, but if by any means the products of combustion are cooled, then the watery vapour is condensed, and gives forth its latent heat; and hence the increased effect, when the boiler already described was used, is not owing merely to the retention of the heat that otherwise would have been lost by radiation, but also to the products of combustion being so much cooled, as to cause the condensation of part of the vapour generated. In using this apparatus, the air from the tube passing through the water, escaped at a temperature varying from 120 to 130, according to the kind of burner and the quantity of gas used; the more rapidly the gas was burned, the higher was the temperature of the air from the escape tube. It is evident then, that, even in this way of applying the heat, much of it was lost; and hence it appeared, that, by transmitting this heated air and steam also through the fluid, there would be a still farther increase of temperature, provided the draught could be kept up, so as to carry off the products of combustion.

To put this to the test, I had another vessel adapted to the boiler; it was merely a tin trough capable of holding half a gallon, through which there was passed a tube that fitted on to the escape tube of the boiler.

When the gas was consumed under the boiler in this way, the boiler containing one gallon, and the other part of the apparatus half a gallon, at 45, by the combustion of 1½ foot of gas, the temperature of the water, when mixed, was 118. The air, as it escaped from the tube, was 70, that of the atmosphere of the room being 58. By adapting another vessel of a similar nature to the preceding, and also containing half a gallon of water, the temperature was still farther elevated.

Using in all 2 gallons of water, by 2 feet of gas, the temperature of the fluid when mixed was 122. The air, as it escaped from the tube, was of the same temperature as that of the room, shewing that the whole of the heat generated by the combustion was taken in by the water, with the exception of that lost by radiation downwards from the burner, which is not great.

With respect to the heat thus lost, it must depend on the mode of combustion. When the flame is small, it may be put far up in the cavity of the boiler; but when it is large, it must be placed lower down, to admit of sufficient current, otherwise there is smoke. The farther up in the cavity the better, provided there is no smoke; but when a small flame is used, owing to the time required, there is a loss of heat from the boiler, so that the one, to a certain extent, counter-balances the other. By having recourse to these contrivances, it is evident that much of the heat that would otherwise escape may be successfully applied; at the same time, for common purposes, these methods are not likely to be adopted, because they make the apparatus complicated, and their use is attended with trouble. A boiler of the form I have already described, may, however, be easily employed, and will answer most of the purposes for which gas, as a source of heat, is likely to be used. With this apparatus, I have found, taking the average of numerous trials, that, by the combustion of 1 foot of gas, 1 gallon of water may be raised from 50 to 105, or even beyond that, consequently 3 feet would bring it up to the boiling point. To boil off the whole of the water would require 5½ times as much as is necessary to raise it from 32 to 212 and $3 \times 5.5 = 16.5$, which added to $3 = 19.5$, but less than this would be required, were the water at 50 instead of 32. In one trial I found that 1 gallon might be boiled off by 16.5 feet, and I consider this as the utmost that can be expected, allowing for the loss always consequent on the use of gas by any of the methods mentioned. Of course, in stating this as the quantity to be evaporated, it applies solely to the gas with which I operated.

From these data we are enabled to ascertain whether gas is, or is not, an economical source of heat. Let us suppose that, on an average, 18 feet would be required to boil off 1 gallon, *i. e.* 10 lb. of water. In this town 1000 feet cost 9s., 18 feet would therefore cost 1.9 of a penny, say 2d.

It is generally allowed, that 1 lb. of coal, were the heat generated during its combustion all applied, would evaporate about 14 lb. of water, but this is never done in practice. It is considered as a good result when it evaporates from 6 to 8 lb. Now, suppose a ton of coal to cost 12s., 1 lb. will cost about the fourth part of a farthing, so that a quantity of coal which costs little more than a fourth of a farthing will evaporate as much water as gas which cost 2d. Perhaps the same proportion will be found to hold at other places, for where coal is dearer gas is also more expensive. But though gas, as a source of heat, is much more expensive than fuel, yet there are occasions where it may be applied where the expense will not be found to be very great; indeed, in some instances not more than would be requisite were coal used, besides having many advantages. When it is required to warm water when there is no fire at hand, a gallon may be boiled by the consumpt of about 3 feet of gas, at a cost, therefore, of little more than a farthing.

With regard to the use of gas for cooking, we are, of course, enabled so far to judge of its expense. In the paper published in the Edinburgh New Philosophical Journal, and in the Transactions of the Society, by Sir J. Robison, already alluded to, it is proposed to burn the gas on gauze of from 3 to 4 inches in diameter; the flame being larger or smaller according to circumstances. Suppose a pot is used holding a gallon of water, and it is wished to bring this to boil in half an hour, and to keep it boiling, then the gauze will consume 6 feet per hour; so that, for each hour the gas is in use, each gauze-burner will cost a little more than a halfpenny, and we may perhaps consider this as an average; the gas being at the rate of 9s. per 1000 feet. Suppose a larger consumpt of gas required, then each burner would cost, say about 1d. per hour; but this is, I believe, beyond what a burner of the kind mentioned is capable of consuming, without waste of gas from smoke, or from part of it escaping without being burned. If six of these burners are in use, say for three hours at a time, then the expense would range from 9d. to 1s. 6d.: say, on an average, about 1s. Of course, the expense will be much diminished, if, instead of keeping the gas burning all the time, at the rate stated, the flame is reduced so as merely to keep the object warm, after bringing it to the due degree.

Now, though this is no great expense, especially considering the cleanliness and other advantages, yet, were it to be used as the only source of heat for cooking, the expense would far exceed that incurred by the use of coal. Where, therefore, economy is an object, I fear that cooking by gas ought not to be recommended; but where convenience and cleanliness are more desired than economy, then, certainly, gas may, for many purposes, be employed, particularly as the expense, keeping in view the quantity used, is by no means great. It has been supposed by some, that, in addition to the expense, another objection against the use of gas for cooking, arises from the time required. In a common pot, half a gallon of water can be made to boil in from five to seven minutes, according to the state of the fire; whereas with gas, suppose the consumpt at the rate of 3 feet per hour, it requires in the same vessel a quarter of an hour; the time being as about three to one. But though there is a difference in the time required for boiling water, there is not the same difference with regard to the cooking. When, for instance, potatoes with one-fourth of a gallon of water, were placed in a pot on a fire, the water was boiled in about five minutes, and in about thirty minutes more, the potatoes were cooked. The same quantity of potatoes and of water when put over a gas-burner, required a quarter of an hour to boil the water, and the same additional time as before for the potatoes being ready. Though, therefore, the time required for boiling the water is greater, yet there is not much difference in the whole time; in fact, it is just the additional time necessary for causing the water to boil.

There is another application of gas as a source of heat, which, so far as I know, has not yet been put in practice, but which, I have every reason to believe, will be found beneficial. I allude to heating water for a bath.

I have already mentioned, that, by the consumpt of a foot of gas, one gallon of water may be raised from 50° to 100°, or a few degrees above it. A slipper-bath holds in general from 25 to 30 gallons. Provided, therefore, the gas can be burned in sufficient quantity, we have an easy method of heating the water. In the trials which I have made, I have used a bath into which were put 24 gallons of water at 50°; beneath the bath, and at a little distance from it, there was passed a tube of about 2 inches diameter, having six rose-jet burners attached to it. The gas was kindled, and in three-quarters of an hour the whole of the water was brought to 100°. The gas consumed was 17 feet, at a cost, therefore of nearly 2d.

I consider this as a much more easy, and altogether, a far superior method to the heating of baths by burning a small fire placed within the bath; for in this case, there must be a tube conveyed to a chimney to carry off the smoke, a part of which, in spite of all our attention, often escapes into the apartment; besides, owing to the great heat immediately over the fire, the water there becomes very warm, and gives off a great deal of steam, which is not the case with the gas, the heat being uniformly applied under the water. I do not mean to recommend this as a method of heating baths for general use; but where a bath is required, as in a bedroom, I consider it an easy and convenient means of procuring one. All that is necessary is to attach a flexible tube to the pipe in the room, and to take care that it is of such a size, that it will supply gas to the extent of from 30 to 40 feet per hour, according as the bath may be required. Six rose-jet burners with sixteen holes each will be sufficient; for each of these will burn about 8 feet per hour. I believe the rose-jets will be found more convenient than gauze, because the gauze requires to be placed on tubes, which would require the bath to be raised to an inconvenient height.

I may mention that, instead of the common bath, I had one constructed with a tube passing through the water, and through which the products of combustion passed, but I did not find much advantage from its use. I would, however, recommend that, instead of having a flat bottom, as is usual, the sides of the bath be extended downwards, and made to contain water, so as to prevent loss of heat by radiation. I am aware that some may object to the use of gas in a bed-room, owing to the production of carbonic acid; but their fears on this account are groundless. Suppose that 40 feet of gas are consumed, and that the whole of the products of combustion escape into the apartment, these will, on an average, yield less than their own bulk of carbonic acid gas, which, when diluted with the air of the apartment, is too small to have any injurious effect on the system.

From what has been said, we are enabled also to ascertain the comparative expense of the method some time ago proposed, of heating apartments, such as churches, manufactories, &c. by gas. This was done by burning the gas as it escaped from a number of small apertures in a circular tube, which was surrounded by a large box of sheet-iron, from which the heated air was conveyed by tubes through the apartment.

It has been already stated, that a pound of coal will evaporate about 14 lb. of water, were the whole heat taken up by the water. But this is never done.

Let us suppose that, in heating a large apartment by a stove throwing in hot air, only one-half of the heat generated by the combustion of coal is available, that is, that as much heat is available as would evaporate only about 7 lb. of water for the pound of coal; then, to produce the same heat by gas, as before stated, the comparative cost of the coal and of the gas would be as one-fourth of a farthing and 2d., that is, in the ratio of 1 to 32.

It is evident from this that gas, as a means of warming apartments, is by far too expensive to allow it to come into use.

That I am correct in what I say, is proved by the results of experiments in which I have been lately engaged in heating apartments by stoves.

In one trial, I found that several large apartments were kept, during winter, at the temperature of about 60°, during twelve hours each day, by the daily consumpt of half a cwt. of coal. Suppose this cost 10s. per ton, then the daily expense is only 3d. But small coal, at from 5s. to 6s. per ton, may be used for this purpose, which would still farther diminish the expense. The cubic area of the apartments alluded to was in all 36,780 feet; now, for 3d. only about 25 feet of gas is purchased, the heat from which would not be more than sufficient to boil off 1½ gallons of water.

Before concluding, I have one observation to make in reference to the heat evolved by coal and by coal-gas, irrespective of the economy.

I have already stated, that it is generally allowed that 1 lb. of coal will evaporate about 14 lb. of water, supposing all the heat evolved were applied. From the experiments of Despretz, it is concluded that, by the combustion of 1 lb. of pure carbon, 12.3 lb. of water will be evaporated. The superior evaporative power of coal must therefore be owing to the bituminous matter which it contains, which bitumen is the source of the gaseous matter evolved when the coal is heated; hence the necessity, when using coal as fuel, of so burning it, that the whole of the gaseous matter evolved during the heating of the coal shall be consumed; for, when any of it escapes combustion, the loss is considerable. I have already mentioned, that 1 gal. water may be boiled off by the consumpt of about from 17 to 20, say, on an average, 18 feet of gas. The quantity of gas which coal affords varies much according to its quality. Of course, the quantity of coke or carbon which coal will leave after being deprived of its gaseous ingredients, must also vary. It is generally allowed that 1 lb. of coal will, on an average, yield about 5 feet of gas, which, at the rate above stated, will evaporate rather more than one-fourth of a gallon, that is 2½ lb.; but, in addition to the gas which is collected by the decomposition of coal, a large quantity of tar and volatile oil is set free, all of which also give forth much heat during their combustion; and hence it is, that the more perfect the combustion of the coal, the greater is its evaporative power; for, while the whole of the coke, which is a fixed principle, is always consumed, with the exception of the cinder, yet more or less of the volatile and gaseous matter may escape, and as these generate a great deal of heat during their combustion, the loss becomes great when much of them is allowed to fly off unburnt.

Beryl Mine of Paddioor, and Geognostic Position of this Gem, in Coimbatoor, Southern India. By Lieutenant Newbold, Madras Army, A. D. C. to Brigadier-General Wilson, C. B. Communicated by the Author.

The Beryl mine of Paddioor, or Patialey as it is sometimes termed, is situated at the eastern extremity of a village of the same name, in the Kanghyum Taluk, in the Coimbatoor Collectorate, about forty miles E.N.E. from the town of Coimbatoor, which lies in Lat. 11° N., and Long. 77° 1′ E. The surrounding country is an undulating plain, studded with a few short clusters of hills, principally of gneiss and quartz rock. The surface of the plain is intersected by a few ravines and rivulets, flowing towards the Noel river, which, rising in the Nilgherry Chain, pursues an easterly course to the Canvery. The latter river flows through the midst of this great plain, which is bounded by the Nilgherries to the west, and the Shevaroy and Salem Mountains to the east. All these ranges consist of normal rocks, principally gneiss, hornblende-slate, and mica-slate, associated with granite, greenstone, and basalt in dykes. The rocks that occur in the plain are of a similar description, with beds of quartz distinctly stratified, sometimes highly ferruginous, and passing into garnet rock. Garnets, regularly crystallized, often occur in the gneiss and hornblende slate. Primitive crystalline limestone and a talcose rock occur, though rarely, interstratified with the gneiss and hornblende slate. A remarkable feature is the prevalence of basaltic dykes, accompanied by large travertine and tufa-like deposits of carbonate of lime. The influence exerted by these dykes over crystalline and metalliferous developments in these districts is interesting and instructive. Dykes, or veins, of a porphyritic granite too, traverse the gneiss in various directions. The larger veins are generally from W. to E., varying a few degrees to the S. of E. The metallic ores and minerals found associated with these rocks are chiefly the magnetic or black iron ore, disseminated and interstratified with quartz rock in a state of great purity. It sometimes occurs in octahedral crystals, with a whitish micaceous looking *enduit.* Both varieties are often highly magnetic with polarity. Manganese also occurs in the form of the black oxide; also garnets, corundum, magnesite, nephrite, asbestus, chromate of iron, adularia, pyrites, &c. Rubies have been found associated with the corundum which occurs imbedded in gneiss. I am not aware that the other beautiful variety of rhomboidal corundum, viz. the sapphire, has ever been discovered; but, from certain indications, I should be led to suppose its existence. Gold-dust is found in many of the rivulets flowing down the sides of the Nilgherry and Salem Mountains.

I will now proceed to describe the bed, and minerals with which the Beryl is more immediately associated in the particular locality which has already been adverted to, viz. Paddioor. The mine has been sunk through a bed or dyke in the gneiss and mica-slate on the line of contact; it is about eighteen paces long, by fourteen broad; it has about seven feet of water covering the lower part, and is about twenty-four deep to the surface of the water. The dyke is composed of a highly crystalline porphyritic granite, the component minerals of which are generally beautifully characteristic and distinct. The quartz is sometimes regularly crystallized, but usually in amorphous translucent masses, imbedded in large tabular crystals of pale rose-coloured felspar, with cleavlandite, garnet, and white, black, and bottle-green mica. A crystallized pyramidal prism of quartz that had been dug out of this mine measured 2 feet 3½ inches in length, and 1 foot 3 inches in diameter; it had, however, been fractured, and four only of the sides were tolerably perfect. The mica occurred both in six-sided tables and in large irregularly-shaped nests, one of which measured 2 feet in length; the laminæ highly elastic and transparent. A few of the garnets were crystallized in dodecahedrons, sparingly disseminated in the rock; one of these crystals measured 2 inches in diameter. The crystals of cleavlandite were remarkably fine, and characteristic of this beautiful variety of felspar. The various minerals composing this bed pass from the porphyritic structure into a curiously fibrous arrangement: the quartz, felspar, and cleavlandite occurring in alternate prismatic laminæ; sections of this rock, at right angles with the long axis of the prisms, exhibit on their surfaces the appearance of *graphic* granite. Where this arrangement is observed, the mica is partially and irregularly distributed in thin pyramidal nests, rarely in direction with the laminæ of the rock. The quartz and felspar, where they meet with a large nest of mica, usually lose their laminar structure, becoming confused and lumpy. The felspar and cleavlandite is both white and translucent, and opaque and reddish; the latter is often crossed by microscopic fissures, inclined at a great angle to the axis of the prism. Ghunpore, in the Nizam's dominions, is the only other locality in India where I have met with this fibrous rock; it was there also associated with gneiss and granite. The cleavlandite often occur in large masses, with small cavities partly formed by the decomposition of the rock, and partly by the intersection of the longer and more distinct crystals of the cleavlandite; it is in this gangue, and in these cavities, that the Beryl, or aquamarine, is almost invariably found, in long deeply striated hexahedral prisms, with small crystals of quartz.

The whole of the rock, composing the dyke, is divided by seams, almost horizontal, intersected by fissures, thus dividing it into cuboidal masses. Many of the seams are filled by a whitish earthy incrustation of carbonate of lime, that has a tendency to collect in nodules. The larger calcareous veins attenuate as they ascend in the rock, and appear to have penetrated it from below, rather than to have been deposited from above. On reaching the surface, which it often overspreads, in beds of great extent, underlying the present soil, it assumes a closer texture, with a nodular or pisiform surface, the interior having a spheroidal structure, not unfrequently assimilating that of the travertine of San Filippo. It has been deposited, no doubt, by springs of water ascending through the subjacent strata, charged with carbonic acid, and holding lime in solution.

Small springs are still observed percolating upwards through the fissures of the rock; and, as they trickle over its surface, depositing a thin black crust composed of carbon, a little iron, calcareous and saline matter; the latter contained both the muriate and sulphate of soda; which salts, and nitrate of potass, exist abundantly in the soil of the adjacent district. The water at the bottom of the mine is sufficiently pure for irrigation and the purposes of life.

There are marks of other excavations in this vicinity, and in the same bed. The natives, however, assert their perfect ignorance of their use. Judging from the high barren situation in which I found them, these excavations could hardly have been made for wells. Blocks of the cleavlandite, which forms the matrix of the gem, had been thrown out, and evidently broken up in search of what it might be supposed to contain. The excavations were shallow, but extensive, and the quantity of the broken up gangue very large; it (the cleavlandite) can be traced in the rocks in the vicinity to an extent, east and west, of about thirty-eight miles; and it is likely that, in ancient times, antecedent to Mahomedan conquest, when rights to landed property were more secure, the gem was obtained in abundance. Now, the surface veins appear to be completely exhausted in all the excavations I examined; but it is probable, from the inquiries I made, that it is still secretly procured by natives from certain localities in this district, as it still forms an article of traffic in Indian bazaars. It has been thought, with some reason, that the largest crystallized Beryl ever known, weighing six ounces, and costing L.500, and which was supposed to have been brought to Europe from Ceylon, was the produce of the Beryl mines of Coimbatoor, as neither Davy, nor any of the authors conversant with the mineralogy of Ceylon, mention its existence on that island. It has been stated positively that it does not occur there.

To Mr Heath the merit is due of having first brought these mines under the notice of government. Mr Fisher, the enterprising land-proprietor of Salem, informed me that the gem was first discovered by the diggers of a well on the estate of the village Potail, who sold them secretly to the itinerant jewellers and chittys, who purchased them in large quantities for a mere trifle, and sold them at an immense profit at Madras, Pondicherry, and other European settlements. This, coming to Mr Heath's knowledge, afforded a clue by which he was enabled to trace the beryl to its situs. He lost no time in obtaining the conditional consent of government, and arranging with the native owner of the land. Mining operations were carried on for about two years, and were discontinued in consequence of the mines being exhausted, and the expense of draining off the water. It has now (1840) reverted to its original purpose of irrigation, and is still the property of the Potail who originally caused it to be dug. This venerable old Hindoo, Chinnana Gouda, whose hair and beard are blanched by age, paid me a visit at the mine. He informed me that it is upwards of forty years ago since he dug it; and that, until Mr Heath's discovery, about twenty-two years ago, he had not the slightest idea of the treasures contained in his own well, and of which he had been robbed for eighteen years by the people he employed.

In India, the beryl appears to be almost confined to this particular district. The natives, however, inform me that it occurs at Vaniambadi, at the northern base of the Nilgherries, in rocks of a similar age and petrographical character. In Europe it occurs also in the primitive rocks—in the granite and gneiss formations.

On the Construction of Circular Towers. By EDWARD SANG, Esq., Civil-Engineer, Edinburgh, M. S. A. Communicated by the Society of Arts.*

SOME years ago I submitted to the Society of Arts an essay on the construction of Oblique Arches, in which the complete theory of cylindroid arches was developed, and the application of that general theory to some particular cases was given. From that inquiry, there resulted several beautiful general propositions concerning the voussoirs of which arches are to be built; in particular, it was found that in a properly constructed arch, where the bed of the stone is proportioned to the strain which it has to bear, the cross sections of the arch stones are all equivalent to each other.

The investigation of the oblique arch is only one case of the general theory of VAULTS; and I would have devoted this paper to that general theory, had I not felt that an abstract generality is best appreciated after a few of its special cases have been examined. In the present paper it is, therefore, proposed to examine another practically important case of vault, namely, that which occurs in building a circular signal tower.

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* Read before the Society of Arts for Scotland 15th April 1840.

Before proceeding to analyze the proper mode of construction, it may be useful to view the defects of the forms hitherto adopted; the utility of the inquiry will be seen, when it is shewn that the common construction introduces into the building one element of self decay.

All the building materials with which we are acquainted consist of granular particles united by a cement, or of crystals closely impacted among each other. The formation of sandstone, and the composition of granite, serve well to exemplify the two classes.

The grains of quartz, of which sandstone is composed, may be regarded as possessing absolute strength, provided their lateral spreading be resisted; thus, if a quantity of sand be put in the bottom of a cylinder, and if a piston be brought down upon it, no conceivable pressure will, so long as the bounding cylinder remains, destroy the particles of sand. Imagine these grains to have been deposited from a gently agitated fluid, and it will appear that each successive particle seeks a position the most conformable with the contiguous ones, so as to give nearly the greatest possible compactness to the whole. After this deposition has taken place, other matters is gradually accumulated above, until the body of sand be subjected to an intense pressure. In this state of things a slow infiltration of water, containing calcareous or siliceous matter, goes on, and there is gradually formed around the grains of sand, and between their contiguous surfaces, a calc or silico-sinter, acting like glue in inducing a cohesion previously awanting.

A slight attention to the circumstances under which this formation takes place, shews that while the cement is formed under the hydrostatic pressure due to the depth of the waters, the sand particles, around which it is placed, are subjected to that pressure, and also to the weight of the superincumbent solid material.

What, then, will be the consequence of removing the sandstone from the position in which it was originally formed?

o soon as the hydrostatic pressure is removed, both the ns and the cement will expand, the expansion being proportional to the height of the hydrostatic column, and to the compressibility of the substance.

When the cement is of the same nature with the grains, both expand alike; and hence the mutual action of the parts is not affected; but when the cement is of a different nature from the grains, the removal of the hydrostatic pressure will occasion one of two classes of effects. If the cement be more compressible than the grains, it will expand more, and separate them somewhat from each other; the aggregate will possess all the cohesive power of the cement. If, on the other hand, the cement be less compressible than the grains, the superior expansiveness of these will, on the removal of the hydrostatic pressure, distend the cement, and the cohesiveness of the mass will be diminished. The sandstone having been dried, let the weight of the superior strata be removed; the cement was not subjected to any strain on account of that weight, it will not expand; but the grains which supported the whole of this weight will expand vertically, and will thus distend the cement, and diminish its cohesion.

In all cases, then, the cohesion across the strata, of a stratified rock, must be diminished by the simple removal of it from its site, while, in some cases, the cohesion in the direction of the strata is also diminished by the mere drying of the rocks.

The accuracy of these inferences is well established by many familiar examples, among which the spontaneous crumbling of various shales is conspicuous. It is to be regretted that we have as yet no accurate experiments on the compressibilities of mineral substances, otherwise the comparative cohesive strengths of various species of stratified rock might be to some extent inferred.

It thus follows, generally, that stratified rocks are capable of but slender resistance to distension; besides, having but a limited range of stretching, they can resist but a very slight blow,—a circumstance which our stone-masons are in daily habit of rendering available, and of which the more conspicuous phases are illustrated in the freestone and the slate quarries.

The formation of igneous or massive rocks is less intelligible; their strengths must necessarily have reference to the material of which they are formed, to the relative thermal expansions of their component parts, and to the compressibility and distensibility of these. When in a state of fusion all the parts of such rocks are subjected to a hydrostatic pressure, so that we may expect less difference of cohesive strengths in various directions than is exhibited in stratified rocks.

In general, then, we arrive at this conclusion, that stones are less able to bear distensions than compressions; that it is nearly indifferent in what direction stones from massive rocks may be placed, but that stones from strata ought always to be so placed that the greatest pressure to which they are subjected may be perpendicular to the plane of stratification.

Keeping these principles in view, let us examine the ordinary construction of signal towers.

These towers are built in horizontal courses, gradually spreading out at the base to resist the greater strain; now here it is apparent that the weight of the central part of the tower must be sustained only by the foundation immediately under it, unless the projecting stones be bent downwards at their inner ends, so as to cause their outer extremities to bear some part of the load placed on them. It is in this way alone that the weight of the structure can be distributed over the extended foundation when the courses are horizontal. The ordinary mode of constructing such towers thus call into action that species of strain which stones are least of all capable of resisting; the inequalities in the mechanical states of the integrant parts has leave to act, and the necessary consequence must be a shortening of the duration of the building. When such towers are subjected to the action of the waves, the outer stones, which resist the first and heaviest blows, are much less securely fastened than otherwise they might be, and thus another imperfection makes its appearance.

The inefficiency of the horizontal courses in preventing dislocation by heavy seas is so obvious, as to have given rise stone introduction of dovetails and dowalls, for the purpose So soeg the stones of one course, and also the different grains ancether. The formation of dovetails occasions a great erial, is expensive in workmanship, and, after all, depends for its efficacy on the worst quality of stone, and on that quality exhibited in its worst form. The dovetail has, in fact, the advantage of a wedge, and tends to break over a piece of stone at its narrow part; nor does the dowall appear under a more promising aspect. I question whether the resistance to dislocation obtained in this way, be comparable with that which results from the friction of one stone upon another under an enormous pressure, assisted as that friction must be by the consolidation of good mortar.

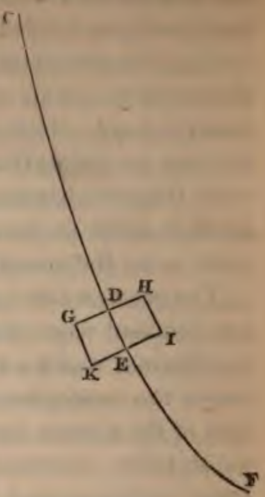
The greatest strength and maximum stability of a structure are obtained when the parts are so formed as that a perfect equilibrium would exist although there were no friction between the contiguous surfaces. The friction and the cohesion of the mortar are then over and above what is needed for stability; whereas, by any other arrangement, part of the latter elements must go to give mere stability, the remaining part only serving to resist extraneous influences.

On the supposition that the whole of the weight is equably distributed over each horizontal section, the form of a tower of solid masonry, which shall have each portion equally pressed on, has long been known to be that of a logarithmic conoid. But it cannot at once be inferred, that the same form will hold when the internal structure of the parts is taken into account. The same form was given by Professor Robison as that required for a suspended rod, and lately Professor Forbes has again proposed it for the shape of Gothic pendants, forgetting, it would appear, that such pendants do not hang by the cohesion of the stones, but that they are built upon an internal rod.

The form which would result as the correct one for a solid tower may not be applicable to the situation and purposes of the building. There is no need for restricting ourselves to this or to any other particular form, since, the form being given, we may set ourselves to inquire what ought to be the mode of construction.

By way of lemma, I shall first consider the law of strain among the parts of a curved wall, regarded as very thin.

Let CDEF be the end view of a curved wall; then, in order that the pressures may be transmitted along the line



CDEF, it is requisite that the joints of the stones be perpendicular to that curve: let GHIK represent one of these stones. The upper surface GH is subjected to a pressure in the direction of the tangent to the curve at the point D; the under surface KI to a pressure upwards in the direction of the curve at the point E. Put p for the pressure at D; $p+dp$ for that at E; put also i for the inclination of GH, $i+di$ for that of KI to the horizon.

The pressure on GH may be decomposed into two, one vertically downwards, represented by $p\ \cos i$, and one horizontally towards the concavity of the curve $p \sin i$. Again, the pressure on KI is decomposed into

$$(p+dp) \cos (i+di)=p \cos i+dp \cos i-p\,di \sin i$$

vertically upwards, and

$$(p+dp) \sin (i+di)=p \sin i+dp \sin i+p\,di \cos i$$

horizontally from the concavity. Let dw be the weight of the stone; then we have, as the sums of all the actions hitherto considered,

$$dw-dp \cos i+p\,di \sin i \text{ upwards,}$$
$$dp \sin i+p\,di \cos i \text{ horizontally}$$

from the concavity.

It thus appears that there cannot be an equilibrium from these three sources, unless i and di be each zero; that is, unless the wall be vertical throughout. In all cases, either of inclined or of curved walls, there must be some means of supplying a pressure from the convex side.

There are three ways in which this pressure may be obtained: two of them relating to a cylindrical wall, such as we have been considering; and one to a wall curved in two directions.

In the *first* place, we may conceive the end GK of the stone to be dressed vertically, and to abut against some firm obstacle, capable of resisting a horizontal thrust. Let that horizontal thrust be dh; then have we,

$$dw - dp \cos i + p\,di \sin i = 0 \quad . \quad . \quad (A.)$$
$$dh - dp \sin i - p\,di \cos i = 0 \quad . \quad . \quad (B.)$$
$$\text{or,} \quad dw = d\,(p \cos i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$
$$dh = d\,(p \sin i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B.)$$

whence, integrating,

$$w \sec i = p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C.)$$
$$h = p \sin i = w \tan i \quad . \quad . \quad . \quad . \quad . \quad (D.)$$

conclusions which might easily have been obtained without the assistance of the integral calculus.

In the *second* place, the stone GHIK may be supposed to abut upon an inclined surface; that is, the wall CDEF may be supposed to be laid into a site ready prepared for it. The retaining pressure is now not horizontal, but inclined to the horizon at the angle $i + \frac{1}{2}di$. Let that pressure be dq; then, decomposed in the vertical direction upwards, it is $dq \, . \sin i$; and in a horizontal direction outwards, it is $dq \cos i$: hence the equations of equilibrium now are,

$$dw = dq \sin i + d\,(p \cos i) \quad . \quad . \quad . \quad (E.)$$
$$0 \; = dq \cos i + d\,(p \sin i) \quad . \quad . \quad . \quad (F.)$$

equations which are no longer integrable without a knowledge of the nature of the curved surface, and of the law according to which the thickness of the wall varies.

As the theory in which I am at present engaged is essentially that of retaining-walls, I may be allowed to pass beyond the lemmatic limits, and to exhibit the actual adaptation of the strength to the strain.

In the first case, when the retaining pressures are horizontal, the investigation presents little or no difficulty. If t be put for the thickness of the wall, and l for the length of the curve, tdl will be proportional to the weight of the stone: indeed, if we take one cubic foot of the stone as the unit of weight, and consider only a length of one foot of wall, $t\partial l$ will take the place of ∂w in equations (A.) and (B.).

Hence we have, supposing p to be proportional to t, that is, $p + nt$,

$$t=\frac{w}{n}\sec i\,;\ \text{or,}\ dw=\frac{1}{n}\,w\,dl\sec i\,;$$

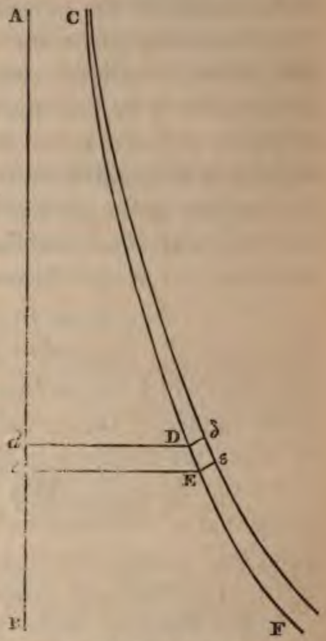
hence $n w^{-1}\,dw=dl\sec i$, and thus $n\,.\,\text{Nap. log}\,w=\int d\,l\sec i$.

On the other hand, when the retaining pressure is perpendicular to the surface of the wall, equations (E.) and (F.) become

$$t\,dl=dq\sin i+n\,d\,(t\cos i)\quad .\ .\quad \text{(G.)}$$
$$0\ =dq\cos i+n\,d\,(t\sin i)\quad .\ .\quad \text{(H.)}$$

by help of which equations the laws of thicknessing in any particular case may be discovered.

To return now to our original subject, that of circular hollow towers:—let AB be the axis of the tower CDEF, the outline of the thin wall which forms it; D$\delta\varepsilon$E one of the stones having its beds Dδ and Eε perpendicular to the curve of the wall. Since the tower is supposed to consist of a mere shell, there is no substance to oppose a resistance on the surface DE: yet, without some outward pressure, the building cannot possibly be stable; how is that resistance to be obtained?



The tower being conoid, the stone may be conceived to be included between two vertical planes passing along the axis AB, and inclined to each other by some angle $\delta\alpha$. The stone will thus be wedge-shaped, and the pressure on its ends, one of which is D$\delta\varepsilon$E, will prevent it from advancing to *de*.

In the case of the straight wall, we considered the action of a portion of it one foot long, that is, included between two parallel vertical planes one foot apart; but in the case of a circular tower, we cannot have recourse to the same expedient, and must take, instead, a portion included between two vertical planes passing through the axis AB, and inclined at some small angle α. It is quite obvious, that the weights of the parts under consideration will be proportional to α, and that, on this account, α must enter as a multiplier.

Putting r for the radius dD, the area of the upper bed of the stone must be $r\alpha t$; and if n be the pressure on each square foot of that area, $nr\alpha t$ must take the place of p in our former investigation, while $r\alpha tdl$ takes place of dw.

The tower being hollow, there is no substance to resist directly the advance of the stone towards de: that resistance is afforded by the pressures against the ends of the stone, or, so to speak, against the two bounding vertical planes. Put σ for the end strain, and each square foot of D$\delta\epsilon$E, and the whole end strain will be σtdl, which, acting at an inclination α, is decomposed into $\sigma tdl \sin \alpha$, in the direction dD; this expression $\sigma tdl \sin \alpha$, or, on account of the smallness of α, $\sigma tdl\alpha$ takes the place of dh; and we thus have

$$w = \int \alpha rtdl = n\alpha rt \cos i \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

$$h = \int \sigma tdl \ = \ n\alpha rt \sin i \quad . \quad . \quad . \quad . \quad \text{(K.)}$$

observing that $\alpha rt = \frac{dw}{dl}$, we find

$$w = n\frac{dw}{dl} \cos i, \text{ or} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(L.)}$$

$$dl \sec i = n\,\frac{dw}{w} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(M.)}$$

as before, so that if the structure be equilibrated, that is, if n be constant, we must have as before

$$n \text{ nep} \log w = \int dl \sec i\,;$$

but the thickness no longer follows the same law, for the resisting surface, being extended in proportion to the radius r, as well as to the thickness t, rt takes now the position which t alone formerly occupied. To fix this point more firmly in the mind, I may remark, that, in the case of a circular tower, the thickness does not necessarily increase with the depth, since the augmentation of the resisting surface, from the greater spread of the circle, may be sufficient to compensate for the increased strain.

In the circular tower, there is a new element, the strain σ on each square foot of the end of the stone. Observing that, in the equation (K.) α is constant, we find

$$\sigma t dl = d\,(n r t \sin i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(N.)}$$

whence, it is evident, that in an equilibrated tower,

$$\sigma dl = n \frac{d\,(r t \sin i)}{t}, \text{ or}$$

$$\sigma dl = \{\sin i\, dr + r \cos i\, di + \frac{r}{t} \sin i dt\} \quad . \quad \text{(O.)}$$

so that the strain σ must increase with the radius of the tower, the outline of the wall remaining the same; and thus the end strain upon the stones of a curved wall is greater with the greater radius of curvature. In practice, it is important to provide that the end tension σ do not exceed the bed tension n: were it to do so, the stones would need to be set up edgeways.

Having now given the general theory of circular towers formed of a single shell, I may now proceed to apply that theory to a few special cases.

1. *Hollow Conical Tower.*

In this case we have i constant, and counting $l = \text{AD}$ from the apex of the cone, we have $r = l \sin i$, and thus in the equilibrated building,

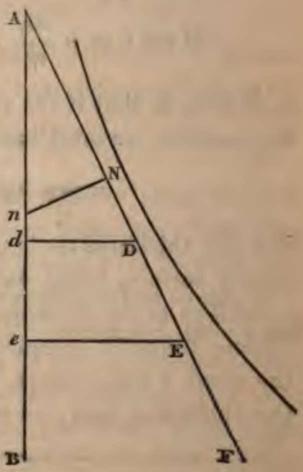
$$n \,.\, \text{nep} \log w = l \sec i + c, \text{ or}$$

$$\text{nep} \log w = \frac{\sec i}{n} l + \frac{c}{n}, \text{ and}$$

thus

$$w = e^{\frac{c + \sec i l}{n}}$$

where the arbitrary constant c must be determined in reference to some starting point of the sys-



tem. The equation may be put in the form

$$w = e^{\frac{c}{n}} \cdot e^{\frac{\sec i \,.\, l}{n}}$$

and on making l zero, that is, on taking the state of matters at the apex of the cone,

$$(w) = e^{\frac{c}{n}}$$

and thus it appears, that, even at the apex of the cone, there must be some extraneous weight. Substituting, we have,

$$w = (w)\, e^{\frac{\sec i \,.\, l}{n}}$$

or, if we put $e^{\frac{\sec i}{n}} = \mathrm{E}$,

$$w = (w)\, \mathrm{E}^l;$$

and thus it appears, that the weights above two given points D and E have their ratio measured by the intervening distance DE.

Having ascertained the entire weight piled above a given point D, we can proceed to inquire how the thickness of the wall is to be regulated so that this arrangement may be brought about.

Differentiating, we have,

$$d w = (w)\, \frac{\sec i}{n}\, \mathrm{E}^l\, dl;$$

but $dw = r \,.\, 2\pi t dl$, taking the weight for the whole circuit of the building, and hence

$$(w)\, \frac{\sec i}{n}\, \mathrm{E}^l = 2\pi r t = 2\pi t l \sin i,$$

or, $$t = \frac{(w)}{n \sin 2i}\, \frac{\mathrm{E}^l}{l} \cdot$$

A glance at the above value will shew that the thickness t has a minimum value. We have in fact

$$dt = \frac{(w)}{n \sin 2i}\, \frac{l \,.\, \frac{\sec i}{n}\, \mathrm{E}^l - \mathrm{E}^l}{l^2} dl$$

$$= \frac{(w)}{n \sin 2i} \frac{\mathrm{E}^{l} \left(l^{\frac{\sec i}{n}} - 1\right)}{l^2} \, dl,$$

and hence t is a minimum when

$$l^{\frac{\sec i}{n}} = 1, \text{ or when } l = n \cos i\,;$$

now, it is to be observed, that n is the height of a column of stone sufficient to produce the actual strain upon any part of the bed, and thus, having laid off this distance $n = \mathrm{A}\,n$, and drawn the perpendicular nN to AF, N is the point at which the thickness of the wall is the least.

2. *Logarithmic Conoid.*

Let (Fig. p. 252.) CDEF be a logarithmic curve, having AB for its axis, then putting $\mathrm{A}d = z$, $r = (r)\, e^{mz}$, (r) being the radius at the top, and m some arbitrary constant. We have now, equation (M.),

$$n \,.\, \text{nep} \log w = \int dl \frac{dl}{dz}$$

$$= \int \frac{dl^2}{dz}$$

now, here, $dr = (r)\, m e^{mz}\, dz$

$\qquad\qquad = r m\, d z\,;$

whence, $\quad dr^2 = r^2 m^2\, dz^2$, and

$\qquad\qquad dl^2 = (r^2 m^2 + 1)\, dz^2$; whence

$$n \,.\, \text{nep} \log w = \int dz \,(r^2 m^2 + 1)$$

$$= \int dz \left((r)^2 e^{2mz} + 1\right)$$

$$= z + (r)^2 \frac{1}{2m} e^{2mz}$$

$$= z + \frac{r^2}{2m} + c.$$

Putting $z = 0$, $w = (w)$, we have

$$n \,.\, \text{nep} \log (w) = 0 + \frac{(r)^2}{2m} + c\,;$$

whence subtracting,

$$n\,.\,\text{nep}\log\frac{w}{(w)} = z + \frac{r^2-(r)^2}{2m}$$

$$= z + \frac{(r)^2}{2m}\left(e^{2mz} - 1\right);$$

hence the logarithm of the ratio of the weight at D to the weight at C consists of two parts, one proportional to the depth Ad, the other proportional to the excess of the area of the horizontal section at D above the area of the horizontal section at C.

Differentiating the above equation, we have

$$n\frac{dw}{w} = \left\{1 + \frac{(r)^2}{2m}\left(2me^{2mz}\right)\right\}dz$$

$$= \left(1 + (r)^2e^{2mz}\right)dz$$

$$= (1 + r^2)\,dz;$$

whence

$$dw = (w)\,e^{\frac{1}{n}\left\{z + \frac{r^2-(r)^2}{2m}\right\}}(1 + r^2)\,dz;$$

so that, taking the weight all round, we have $dw = 2\pi rt\,dl = 2\pi rt\sqrt{(r^2m^2 + 1)}\,.\,dz$, or

$$t = we^{\frac{1}{n}\left\{z + \frac{r^2-(r)^2}{2m}\right\}}\frac{1 + r^2}{\pi r\sqrt{(1 + m^2r^2)}}.$$

3. *Parabolic Towers.*

In the first place, I shall consider the case of a conoid formed by causing a parabola to revolve upon its axis; the tower will then present a convex appearance.

In this case, we have

$$2fr = z^2, \quad \text{or}$$

$$2f\,dr = z\,dz$$

$$dr^2 = \frac{z^2}{4f^2}\,dz^2$$

$$dl^2 = \frac{4f^2 + z^2}{4f^2}\,dz^2;$$

whence

$$n\ \text{nep}\log w = \int\left(1 + \frac{z^2}{4f^2}\right)dz$$

$$= z + \frac{1}{1z}\frac{z^3}{f^2} + c.$$

Here we observe at once that

$$n \text{ nep log } (w) = c; \text{ whence,}$$

$$n \text{ nep log } \frac{w}{(w)} = z + \frac{z^3}{12 f^2}.$$

Now, taking the weight all round,

$$dw = 2\pi \frac{z^2}{4f} t dl; \text{ while, from the above equation,}$$

$$w = e^{\frac{1}{n}\left(z + \frac{z^3}{12 f^2}\right)}$$

$$dw = \frac{1}{n}\left(1 + \frac{z^2}{4f^2}\right) e^{\frac{1}{n}\left(z + \frac{z^3}{12 f^2}\right)} dz,$$

and $dl = \frac{\sqrt{4f^2 + z^2}}{2f^2} dz$

$$dw = \frac{\pi z^2 t \sqrt{4f^2 + z^2}}{4f^2} dz; \text{ so that}$$

$$t = \frac{\frac{1}{n}\left(1 + \frac{z^2}{4f^2}\right) e^{\frac{1}{n}\left(z + \frac{z^3}{12 f^2}\right)}}{\frac{\pi z^2}{4f^2}\sqrt{4f^2 + z^2}}$$

$$t = \frac{\sqrt{4f^2 + z^2} \cdot w}{\pi z^2 (w)}.$$

This expression gives the thickness infinite at the summit.

Of the Relation of Tradition to Palætiology. By the Rev. William Whewell, B. D.*

1. *Importance of Tradition.*—Since the Palætiological Sciences have it for their business to study the train of past events produced by natural causes down to the present time, the knowledge concerning such events which is supplied by the remembrance and records of man, in whatever form, must have

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* From Professor Whewell's valuable work, entitled, "The Philosophy of *the Inductive* Sciences founded upon their History." 1840. Vol. 2.

an important bearing upon these sciences. All changes in the condition and extent of land and sea, which have taken place within man's observation, all effects of deluges, sea-waves, rivers, springs, volcanos, earthquakes, and the like, which come within the reach of human history, have a strong interest for the palætiologist. Nor is he less concerned in all recorded instances of the modification of the forms and habits of plants and animals, by the operations of man, or by transfer from one land to another. And when we come to the Palætiology of Language, of Art, of Civilization, we find our subject still more closely connected with history; for in truth these are historical, no less than palætiological investigations. But, confining ourselves at present to the material sciences, we may observe, that, though the importance of the information which tradition gives us, in the sciences now under our consideration, as, for instance geology, has long been tacitly recognised; yet it is only recently that geologists have employed themselves in collecting their historical facts upon such a scale and with such comprehensive views as are required by the interest and use of collections of this kind. The Essay of Von Hoff,* *On the Natural Alterations in the Surface of the Earth which are proved by Tradition*, was the work which first opened the eyes of geologists to the extent and importance of this kind of investigation. Since that time the same path of research has been pursued with great perseverance by others, especially by Mr Lyell; and is now justly considered as an essential portion of geology.

2. *Connection of Tradition and Science.*—Events which we might naturally expect to have some bearing on geology, are recorded in the historical writings which, even on mere human grounds, have the strongest claim to our respect as records of the early history of the world, and are confirmed by the traditions of various nations all over the globe, namely, the formation of the earth and its population, and a subseqnent deluge. It has been made a matter of controversy how the narrative of these events is to be understood, so as to make it agree with the facts which an examination of the earth's surface and of its vegetable and animal population discloses to us. Such controversies, when they are considered as merely arch-

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* Vol. i., 1822; vol. ii., 1824.

æological, may occur in any of the palætiological sciences. We may have to compare and to reconcile the evidence of existing phenomena with that of historical tradition. But under some circumstances this process of conciliation may assume an interest of another kind, on which we will make a few remarks.

3. *Natural and Providential History of the World.*—We may contemplate the existence of Man upon the earth, his origin and his progress, in the same manner as we contemplate the existence of any other race of animals; namely, in a purely palætiological view. We may consider how far our knowledge of laws of causation enables us to explain his diffusion and migration, his differences and resemblances, his actions and works. And this is the view of man as a member of the *natural* course of things.

But man, at the same time the contemplator and the subject of his own contemplation, endowed with faculties and powers which make him a being of a different nature from other animals, cannot help regarding his own actions and enjoyments, his recollections and his hopes, under an aspect quite different from any that we have yet had presented to us. We have been endeavouring to place in a clear light the Fundamental Ideas, such as that of Cause, on which depends our knowledge of the natural course of things. But there are other Ideas to which man necessarily refers his actions; he is led by his nature, not only to consider his own actions, and those of his fellow-men, as springing out of this or that cause, leading to this or that material result; but also as good or bad, as what they ought or ought not to be. He has Ideas of *moral* relations as well as those Ideas of material relations with which we have hitherto been occupied. He is a moral as well as a natural agent.

Contemplating himself and the world around him by the light of his Moral Ideas, man is led to the conviction that his moral faculties were bestowed upon him by design and for a purpose; that he is the subject of a moral government; that the course of the world is directed by the Power which governs it, to the unfolding and perfecting of man's moral nature; that this guidance may be traced in the career of individuals and of the world; that there is a *providential* as well as a *natural* course of things.

Yet this view is beset by no small difficulties. The full development of man's moral faculties;—the perfection of his nature up to the measure of his own ideas;—the adaptation of his moral being to an ultimate destination, by its transit through a world full of moral evil, in which each has his share; —are effects for which the economy of the world appears to contain no adequate provision. Man, though aware of his moral nature, and ready to believe in an ultimate destination of purity and blessedness, is too feeble to resist the temptation of evil, and to restore his purity when once lost. He cannot but look for some confirmation of that providential order which he has begun to believe; some provision for those deficiencies in his moral condition which he has begun to feel.

He looks at the history of the world, and he finds that at a certain period it offers to him the promise of what he seeks. When the natural powers of man had been developed to their full extent, and were beginning to exhibit symptoms of decay; when the intellectual progress of the world appeared to have reached its limit, without supplying man's moral needs; we find the great Epoch in the Providential history of the world. We find the announcement of a Dispensation by which man's deficiencies shall be supplied and his aspirations fulfilled: we find a provision for the purification, the support, and the ultimate beatification of those who use the provided means. And thus the providential course of the world becomes consistent and intelligible.

4. *The Sacred Narrative.*—But with the new Dispensation, we receive, not only an account of its own scheme and history, but also a written narrative of the providential course of the world from the earliest times, and even from its first creation. This narrative is recognised and authorised by the new dispensation, and accredited by some of the same evidences as the dispensation itself. That the existence of such a sacred narrative should be a part of the providential order of things, cannot but appear natural; but naturally also, the study of it leads to some difficulties.

The sacred narrative in some of its earliest portions speaks of natural objects and occurrences respecting them. In the very beginning of the course of the world, we may readily believe (indeed as we have seen in the last chapter, our scientific researches lead us to believe) that such occurrences were very different from anything which now takes place;—different to an extent and in a manner which we cannot estimate. Now the narrative must speak of objects and occurrences in the words and phrases which have derived their meaning from their application to the existing natural state of things. When applied to an initial supernatural state, therefore, these words and phrases cannot help being to us obscure and mysterious, perhaps ambiguous and seemingly contradictory.

5. *Difficulties in interpreting the Sacred Narrative.*—The moral and providential relations of man's condition are so much more important to him than mere natural relations, that at first we may well suppose he will accept the Sacred Narrative, as not only unquestionable in its true import, but also as a guide in his views even of mere natural relations. He will try to modify the conceptions which he entertains of objects and their properties, so that the Sacred Narrative of the supernatural condition shall retain the first meaning which he had put upon it, in virtue of his own habits in the usage of language.

But man is so constituted that he cannot persist in this procedure. The powers and tendencies of his intellect are such that he cannot help trying to attain true conceptions of objects and their properties by the study of things themselves. For instance, when he at first read of a firmament dividing the waters above from the waters below, he perhaps conceived a transparent floor in the skies, on which the superior waters rested which descend in rain; but as his observations and his reasonings satisfied him that such a floor could not exist, he became willing to allow (as St Augustine allowed) that the waters above the firmament are in a state of vapour. And in like manner in other subjects, men, as their views of nature became more distinct and precise, modified, so far as it was necessary for consistency's sake, their first rude interpretations of the Sacred Narrative; so that, without in any degree losing its import as a view of the providential course of the world, it should be so conceived as not to contradict what they knew of the natural order of things.

But this accommodation was not always made without *painful struggles* and angry controversies. When men had conceived the occurrences of the Sacred Narrative in a particular manner, they could not readily and willingly adopt a new mode of conception; and they resisted all attempts to recommend it to them, as attacks upon the sacredness of the Narrative. They had clothed their belief of the workings of Providence in certain images; and they clung to those images with the persuasion that without them their belief could not subsist. Thus they imagined to themselves that the earth was a flat floor, solidly and broadly laid for the convenience of man, and they felt as if the kindness of Providence was disparaged, when it was maintained that the earth was a globe held together only by the mutual attraction of its parts.

The most memorable instance of a struggle of this kind is to be found in the circumstances which attended the introduction of the Heliocentric Theory of Copernicus to general acceptance. On this controversy I have already made some remarks in the History of Science,* and have attempted to draw from it some lessons which may be useful to us when any similar conflict of opinions may occur. I will here add a few reflections with a similar view.

6. *Such difficulties inevitable.*—In the first place, I remark that such modifications of the current interpretation of the words of Scripture appear to be an inevitable consequence of the progressive character of Natural Science. Science is constantly teaching us to describe known facts in new language, but the language of Scripture is always the same. And not only so, but the language of Scripture is necessarily adapted to the common state of man's intellectual development, in which he is supposed not to be possessed of science. Hence the phrases used by Scripture are precisely those which science soon teaches man to consider as inaccurate. Yet they are not on that account the less fitted for their proper purpose: for if any terms had been used, adapted to a more advanced state of knowledge, they must have been unintelligible among those to whom the Scripture was first addressed. If the Jews had been told that water existed in the clouds in small drops, they would have marvelled that it did not constantly descend; and to have explained the reason of this, would have been to teach

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* Vol. i. 401.

Atmology in the Sacred Writings. If they had read in their Scripture that the earth was a sphere, when it appeared to be a plain, they would only have been disturbed in their thoughts or driven to some wild and baseless imaginations by a declaration to them so strange. If the Divine Speaker, instead of saying that he would set his bow in the clouds, had been made to declare that he would give to water the property of refracting different colours of different angles, how utterly unmeaning to the hearers would the words have been! And in these cases, the expressions, being unintelligible, startling, and bewildering, would have been such as tended to unfit the Sacred Narrative for its place in the providential dispensation of the world.

Accordingly, in the great controversy which took place in Galileo's time between the defenders of the then customary interpretations of Scripture, and the assertors of the Copernican system of the universe, when the innovators were upbraided with maintaining opinions contrary to Scripture, they replied that Scripture was not intended to teach men astronomy, and that it expressed the acts of divine power in images which were suited to the ideas of unscientific men. To speak of the rising and setting and travelling of the sun, of the fixity and of the foundations of the earth, was to use the only language which would have made the Sacred Narrative intelligible. To extract from these and the like expressions doctrines of science, was, they declared, in the highest degree unjustifiable; and such a course could lead, they held, to no result but a weakening of the authority of Scripture in proportion as its credit was identified with that of these modes of applying it. And this judgment has since been generally assented to by those who most reverence and value the study of the designs of Providence as well as that of the works of nature.

7. *Science tells us nothing concerning Creation.*—Other apparent difficulties arise from the accounts given in the Scripture of the first origin of the world in which we live: for example, light is represented as created before the sun. With regard to difficulties of this kind, it appears that we may derive some instruction from the result to which we were led in the last chapter;—namely, that in the sciences which trace the progress of natural occurrences, we can in no case go back to an

origin, but in every instance appear to find ourselves separated from it by a state of things, and an order of events, of a kind altogether different from those which come under our experience. The thread of induction respecting the natural course of the world snaps in our fingers, when we try to ascertain where its beginning is. Since, then, science can teach us nothing positive respecting the beginning of things, she can neither contradict nor confirm what is taught by Scripture on that subject; and thus, as it is unworthy timidity to fear contradiction, so is it ungrounded presumption to look for confirmation in such cases. The providential history of the world has its own beginning, and its own evidence; and we can only render the system insecure, by making it lean on our material sciences. If any one were to suggest that the nebular hypothesis countenances the Scripture history of the formation of this system, by shewing how the luminous matter of the sun might exist previous to the sun itself, we should act wisely in rejecting such an attempt to weave together these two heterogeneous threads; —the one a part of a providential scheme, the other a fragment of physical speculation.

We shall best learn those lessons of the true philosophy of science which it is our object to collect, by attending to portions of science which have gone through such crises as we are now considering; nor is it requisite, for this purpose, to bring forward any subjects which are still under discussion. It may, however, be mentioned that such maxims as we are now endeavouring to establish, and the one before us in particular, bear with a peculiar force upon those Palætiological Sciences of which we have been treating in the present Book.

8. *Scientific views, when familiar, do not disturb the authority of Scripture.*—There is another reflection which may serve to console and encourage us in the painful struggles which thus take place, between those who maintain interpretations of Scripture already prevalent, and those who contend for such new ones as the new discoveries of science require. It is this; —that though the new opinion is resisted by one party as something destructive of the credit of Scripture and the reverence which is its due, yet, in fact, when the new interpretation has been generally established and incorporated with men's current thoughts, it ceases to disturb their views of the autho-

rity of the Scripture or of the truth of its teaching. When the language of Scripture, invested with its new meaning, has become familiar to men, it is found that the ideas which it calls up are quite as reconcilable as the former ones were with the most entire acceptance of the providential dispensation. And when this has been found to be the case, all cultivated persons look back with surprise at the mistake of those who thought that the essence of the revelation was involved in their own arbitrary version of some collateral circumstance in the revealed narrative. At the present day, we can hardly conceive how reasonable men could ever have imagined that religious reflections on the stability of the earth, and the beauty and use of the luminaries which revolve round it, would be interfered with by an acknowledgment that this rest and motion are apparent only.* And thus the authority of revelation is not shaken by any changes introduced by the progress of science in the mode of interpreting expressions which describe physical objects and occurrences; provided the new interpretation is admitted at a proper season, and in a proper spirit; so as to soften, as much as possible, both the public controversies and the private scruples which almost inevitably accompany such an alteration.

9. *When should old Interpretations be given up?*—But the question then occurs, What is the proper season for a religious and enlightened commentator to make such a change in the current interpretation of sacred Scripture? At what period ought the established exposition of a passage to be given up, and a new mode of understanding the passage, such as is, or seems to be, required by new discoveries respecting the laws of nature, accepted in its place? It is plain, that to introduce such an alteration lightly and hastily would be a procedure fraught with inconvenience; for if the change were made in such a manner, it might be afterwards discovered that it had been adopted without sufficient reason, and that it was necessary to reinstate the old expositions. And the minds of the readers of Scripture, always to a certain extent and for a time disturbed by the subversion of their long-established notions, would be distressed without any need, and might be seriously

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* I have here borrowed a sentence or two from my own History.

unsettled. While, on the other hand, a too protracted and obstinate resistance to the innovation, on the part of the scriptural expositors, would tend to identify, at least in the minds of many, the authority of the Scripture with the truth of the exposition; and therefore would bring discredit upon the revealed word, when the established interpretation was finally proved to be untenable.

A rule on this subject, propounded by some of the most enlightened dignitaries of the Roman Catholic church, on the occasion of the great Copernican controversy begun by Galileo, seems well worthy of our attention. The following was the opinion given by Cardinal Bellarmine at the time:—"When a *demonstration* shall be found to establish the earth's motion, it will be proper to interpret the sacred Scriptures otherwise than they have hitherto been interpreted in those passages where mention is made of the stability of the earth and movement of the heavens." This appears to be a judicious and reasonable maxim for such cases in general. So long as the supposed scientific discovery is doubtful, the exposition of the meaning of Scripture given by commentators of established credit is not wantonly to be disturbed: but when a scientific theory, irreconcilable with this ancient interpretation, is clearly proved, we must give up the interpretation, and seek some new mode of understanding the passage in question, by means of which it may be consistent with what we know; for if it be not, our conception of the things so described is no longer consistent with itself.

It may be said that this rule is indefinite, for who shall decide when a new theory is completely demonstrated, and the old interpretation become untenable? But to this we may reply, that if the rule be assented to, its application will not be very difficult. For when men have admitted as a general rule, that the current interpretations of scriptural expressions respecting natural objects and events may possibly require, and in some cases certainly will require, to be abandoned, and new ones admitted, they will hardly allow themselves to contend for such interpretations as if they were essential parts of revelation; and will look upon the change of exposition, whether it come sooner or later, without alarm or anger. And when men lend themselves to the progress of truth in this spirit, it is not of any material importance at what period a new and satisfactory interpretation of the scriptural difficulty is found; since a scientific exactness in our apprehension of the meaning of such passages as are now referred to is very far from being essential to our full acceptance of revelation.

10. *In what Spirit should the Change be accepted?*—Still these revolutions in scriptural interpretation must always have in them something which distresses and disturbs religious communities. And such uneasy feelings will take a different shape, according as the community acknowledges or rejects a paramount interpretative authority in its religious leaders. In the case in which the interpretation of the Church is binding upon all its members, the more placid minds rest in peace upon the ancient exposition, till the spiritual authorities announce that the time for the adoption of a new view has arrived; but in these circumstances, the more stirring and inquisitive minds, which cannot refrain from the pursuit of new truths and exact conceptions, are led to opinions which, being contrary to those of the Church, are held to be sinful. On the other hand, if the religious constitution of the community allow and encourage each man to study and interpret for himself the Sacred Writings, we are met by evils of another kind. In this case, although, by the unforced influence of admired commentators, there may prevail a general agreement in the usual interpretation of difficult passages, yet as each reader of the Scripture looks upon the sense which he has adopted as being his own interpretation, he maintains it, not with the tranquil acquiescence of one who has deposited his judgment in the hands of his Church, but with the keenness and strenuousness of self-love. In such a state of things, though no judicial severities can be employed against the innovators, there may arise more angry controversies than in the other case.

It is impossible to overlook the lesson which here offers itself, that it is in the highest degree unwise in the friends of religion, whether individuals or communities, unnecessarily to embark their credit in expositions of Scripture on matters which appertain to natural science. By delivering physical doctrines as the teaching of revelation, religion may lose much, but cannot gain anything. This maxim of practical wisdom has often been urged by Christian writers. Thus St

Augustin says:[*] "In obscure matters and things far removed from our senses, if we read anything, even in the divine Scripture, which may produce diverse opinions without damaging the faith which we cherish, let us not rush headlong by positive assertion to either the one opinion or the other; lest, when a more thorough discussion has shewn the opinion which we had adopted to be false, our faith may fall with it: and we should be found contending, not for the doctrine of the sacred Scriptures, but for our own; endeavouring to make our doctrine to be that of the Scriptures, instead of taking the doctrine of the Scriptures to be ours." And in nearly the same spirit, at the time of the Copernican controversy, it was thought proper to append to the work of Copernicus a postil, to say that the work was written to account for the phenomena, and that people must not run on blindly and condemn either of the opposite opinions. Even when the Inquisition, in 1616, thought itself compelled to pronounce a decision upon this subject, the verdict was delivered in very moderate language:—that "the doctrine of the earth's motion appeared to be contrary to Scripture:" and yet, moderate as this expression is, it has been blamed by judicious members of the Roman church as deciding a point such as religious authorities ought not to pretend to decide; and has brought upon that church no ordinary weight of general condemnation. Kepler pointed out, in his lively manner, the imprudence of employing the force of religious authorities on such subjects: *Acies dolabræ in ferrum illisa, postea nec in lignum valet amplius. Capiat hoc cujus interest.* "If you *will* try to chop iron, the axe becomes unable to cut even wood."

11. *In what Spirit should the Change be urged?*—But while we thus endeavour to shew in what manner the interpreters of Scripture may most safely and most properly accept the discoveries of science, we must not forget that there may be errors committed on the other side also; and that men of science, in bringing forward views which may for a time disturb the minds of lovers of Scripture, should consider themselves as bound by

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[*] Lib. i. *de Genesi*, cap. 18.

strict rules of candour, moderation, and prudence. Intentionally to make their supposed discoveries a means of discrediting, contradicting, or slighting the sacred Scriptures, or the authority of religion, is in them unpardonable. As men who make the science of Truth the business of their lives, and are persuaded of her genuine superiority, and certain of her ultimate triumph, they are peculiarly bound to urge her claims in a calm and temperate spirit; not forgetting there are other kinds of truth besides that which they peculiarly study. They may properly reject authority in matters of science; but they are to leave it its proper office in matters of religion. I may here again quote Kepler's expressions: "In Theology we balance authorities, in Philosophy we weigh reasons. A holy man was Lactantius who denied that the earth was round; a holy man was Augustin, who granted the rotundity, but denied the antipodes; a holy thing to me is the Inquisition, which allows the smallness of the earth, but denies its motion; but more holy to me is Truth; and hence I prove, from philosophy, that the earth is round, and inhabited on every side, of small size, and in motion among the stars,—and this I do with no disrespect to the Doctors." I the more willingly quote such a passage from Kepler, because the entire ingenuousness and sincere piety of his character does not allow us to suspect in him anything of hypocrisy or latent irony. That similar professions of respect may be made ironically, we have a noted example in the celebrated Introduction to *Galileo's Dialogue on the Copernican System;* probably the part which was most offensive to the authorities. "Some years ago," he begins, "a wholesome edict was promulgated at Rome, which, in order to check the perilous scandals of the present age, imposed silence upon the Pythagorean opinion of the mobility of the earth. There were not wanting," he proceeds, "persons who rashly asserted that this decree was the result, not of a judicious inquiry, but of passion ill-informed; and complaints were heard that counsellors, utterly unacquainted with astronomical observation, ought not to be allowed, with their sudden prohibitions, to clip the wings of speculative intellects. *At the hearing of rash lamentations like these, my zeal could not keep silence.*" And he then goes on to say, that he wishes, in his *Dialogue*, to shew that the subject had been fully examined at Rome. Here the irony is quite transparent, and the sarcasm glaringly obvious. I think we may venture to say that this is not the temper in which scientific questions should be treated; although by some, perhaps, the prohibition of public discussion may be considered as justifying any evasion which is likely to pass unpunished.

12 *Duty of Mutual Forbearance.*—We may add, as a further reason for mutual forbearance in such cases, that the true interests of both parties are the same. The man of science is concerned, no less than any other person, in the truth and import of the divine dispensation; the religious man, no less than the man of science, is, by the nature of his intellect, incapable of believing two contradictory declarations. Hence they have both alike a need for understanding the Scripture in some way in which it shall be consistent with their understanding of nature. It is for their common advantage to conciliate, as Kepler says, the finger and the tongue of God, his works and his word. And they may find abundant reason to bear with each other, even if they should adopt for this purpose different interpretations, each finding one satisfactory to himself; or if any one should decline employing his thoughts on such subjects at all. I have elsewhere* quoted a passage from Kepler† which appears to me written in a most suitable spirit: "I beseech my reader that, not unmindful of the Divine goodness bestowed upon man, he do with me praise and celebrate the wisdom of the Creater, which I open to him from a more inward explication of the form of the world, from a searching of causes, from a detection of the errors of vision; and that thus not only in the firmness and stability of the earth may we perceive with gratitude the preservation of all living things in nature as the gift of God: but also that in its motion, so recondite, so admirable, we may acknowledge the wisdom of the Creator. But whoever is too dull to receive this science, or too

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* *Bridgewater Tr.*, b. 314. † *Com. Stell. Mart.*, Introd.

weak to believe the Copernican system without harm to his piety, him, I say, I advise that, leaving the school of astronomy, and condemning, if so he please, any doctrines of the philosophers, he follow his own path, and desist from this wandering through the universe; and that, lifting up his natural eyes with which alone he can see, he pour himself out from his own heart in worship of God the Creator, being certain that he gives no less worship to God than the astronomer, to whom God has given to see more clearly with his inward eyes, and who, from what he has himself discovered, both can and will glorify God."

13. *Case of Galileo.*—I may perhaps venture here to make a remark or two upon this subject, with reference to a charge brought against a certain portion of the *History of the Inductive Sciences.* Complaint has been made* that the character of the Roman church, as shewn in its behaviour towards Galileo, is misrepresented in the account given of it in the History of Astronomy. It is asserted that Galileo provoked the condemnation he incurred; first, by pertinaciously demanding the assent of the ecclesiastical authorities to his opinion of the consistency of the Copernican doctrine with Scripture; and afterwards by contumaciously, and, as we have seen, contumeliously violating the silence which the Church had enjoyed upon him. It is further declared, that the statement which represents it as the habit of the Roman church to dogmatize on points of natural science is unfounded; as well as the opinion that, in consequence of this habit, new scientific truths were promulgated less boldly in Italy than in other countries. I shall reply very briefly on these subjects; for the decision of them is by no means requisite in order to establish the doctrines to which I have been led in the present chapter, nor, I hope, to satisfy my reader that my views have been collected from an impartial consideration of scientific history.

With regard to Galileo, I do not think it can be denied that he obtruded his opinions upon the ecclesiastical authorities in an unnecessary and imprudent manner. He was of an ardent character, strongly convinced himself, and urged on still more by the conviction which he produced among his disciples, and thus he became impatient for the triumph of truth. This judgment of him has recently been delivered by various independent authorities, and has undoubtedly considerable foundation.* As to the question whether authority in matters of natural science were habitually claimed by the authorities of the Church of Rome, I have to allow that I cannot produce instances which establish such a habit. We, who have been accustomed to have daily before our eyes the Monition which the Romish editors of Newton thought it necessary to prefix—*Cæterum latis a summo Pontifice contra telluris motum Decretis, nos obsequi profitemur*—were not likely to conjecture that this was a solitary instance of the interposition of the Papal authority on such subjects. But although it would be easy to find declarations of heresy delivered by Romish Universities, and writers of great authority, against tenets belonging to the natural sciences, I am not aware that any other case can be adduced in which the Church or the Pope can be shewn to have pronounced such a sentence. I am well contented to acknowledge this; for I should be far more gratified by finding myself compelled to hold up the seventeenth century as a model for the nineteenth in this respect, than by having to sow enmity between the admirers of the past and the present through any disparaging contrast.†

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* *Dublin Review,* No. ix., July 1838, p. 72.

With respect to the attempt made in my History to characterise the intellectual habits of Italy as produced by her religious condition,—certainly it would ill become any student of the history of science to speak slightingly of that country, always the mother of the sciences, always ready to catch the dawn and hail the rising of any new light of knowledge. But I think our admiration of this activity and acuteness of mind is by no means inconsistent with the opinion, that new truths were promulgated more boldly beyond the Alps, and that the subtilty of the Italian intellect loved to insinuate what the rough German bluntly asserted. Of the decent duplicity with which forbidden opinions were handled, the reviewer himself gives us instances, when he boasts of the liberality with which Copernican professors were placed in important stations by the ecclesiastical authorities, soon after the doctrine of the motion of the earth had been declared by the same authorities contrary to Scripture. And in the same spirit is the process of demanding from Galileo a public and official recantation of opinions, which he had repeatedly been told by his ecclesiastical superiors he might hold as much as he pleased. I think it is easy to believe that, among persons so little careful to reconcile public profession with private conviction, official decorum was all that was demanded. When Galileo had made his renunciation of the earth's motion on his knees, he rose and said, as we are told, *E pur si muove*—" and yet it *does* move." This is sometimes represented as the heroic soliloquy of a mind cherishing its conviction of the truth, in spite of persecution; I think we may more naturally conceive it uttered as a playful epigram in the ear of a cardinal's secretary, with a full knowledge that it would be immediately repeated to his master.

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* Besides the *Dublin Review*, I may quote the *Edinburgh Review*, which I suppose will not be thought likely to have a bias in favour of the exercise of ecclesiastical authority in matters of science; though certainly there is a puerility in the critic's phraseology which does not add to the weight of his judgment. "Galileo contrived to surround the truth with every variety of obstruction. The tide of knowledge, which had hitherto advanced in peace, he crested with angry breakers, and he involved in its surf both his friends and his foes."—*Ed. Rev.*, No. cxxiii. p. 126.

† I may add that the most candid of the adherents of the Church of Rome condemn the assumption of authority in matters of science made, in this one instance at least, by the ecclesiastical tribunals. The author of the *Ages of Faith* (Book viii. p. 248), says, "A Congregation, it is to be lamented, declared the new system to be opposed to Scripture, and therefore heretical."

On some Phenomena of the Diluvian Epoch. By M. Studer.

The members of the Geological Society (of France) who were present at the meeting at Porrentruy, will doubtless recollect the calcareous rocks of Neuveville, polished and marked with furrows and striæ, on which M. Agassiz has partly founded his argument in favour of a period when ice covered the entire surface of the earth. Granting all the importance which is due to this opinion, whether in the theory of MM. Venetz and Charpentier, who simply admit the former extension of our glaciers to the foot of Jura, or in that of M. Agassiz; and an inspection of polished and striated rocks near actually existing glaciers, being in some measure a necessary addition to our observations on the lake of Bienne; it afforded me the highest gratification to make a journey, this summer, to Mount Rosa, in company with M. Agassiz and many of our friends who took part in the discussion the preceding autumn.* We visited in company Zermatt, in the bottom of the valley of St Nicholas in the Valais, and devoted two days to the examination of the extensive and magnificent glaciers which descend from Monte Rosa and Mont Cervin, towards the bottom of this great amphitheatre surrounded by the highest peaks of the Alps. After these excursions, we separated, and I then made a journey round Monte Rosa and Mont Blanc, passing by Monte Moro, Val Sejia, Ivrea, Col du Bonhomme, Sixt, and Evian. Although the study of rocks was the principal object of this journey, I did not fail to attend to every thing calculated to throw more light on the great question of diluvial phenomena, and the transportation of rolled masses of rock. Convinced that we are still deficient in data to enable us to come to a determinate opinion on this difficult problem, I do not hesitate to add my small contribution to the general stock, by presenting to the Society my observations relating to these subjects. Should they be considered of little importance, they will at all events escape the imputation of having been influenced by my too great predilection for any particular glacial system, whatsoever name it may bear.

We commenced at Zermatt with the examination of the rocks in contact with the great glacier of Gorneren, the lower part of which is formed by the union of the glaciers of the Cima de Jazi, Monte Rosa, and Breithorn. When we had ascended about fifty feet on the right or eastern margin of the glacier, it was in such a state as to allow us to approach near its contact with the rock in situ, and to observe the state of the latter even under the glacier. Notwithstanding the mineralogical difference of the rock, which is here a rather compact green slate, I must say that I was struck with the perfect resemblance of the condition of the surface with that of the calcareous rocks of the lake of Bienne; there are the same smooth spaces, the same grooves with smooth edges, and the same fine striæ; the whole undoubtedly produced by the friction on the rock of boulders and gravel urged forward under a strong pressure by some agent or other, and in this locality that agent may well be considered the glacier itself.

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* For a notice of this tour, by M. Agassiz, see last number, p. 184.—EDIT.

On the following day we ascended the crest of the Riffel, which overlooks the upper part of the glacier of the Gorneren, in the continuation of its right edge. There we were elevated about 500 feet above its surface, and separated from it by a very steep, somewhat peak-shaped, declivity. The prevailing rock of the crest is an imperfectly slaty serpentine. The height of this place above the glacier prevents us supposing that the glacier ever rose so high, since the commencement of the present epoch; and yet we saw the surface of the serpentine rocks polished like a mirror and covered with furrows and striæ nearly horizontal, and in every respect resembling those in contact with the glacier itself. The supposition of currents of water carrying along stones, to which this condition of the surface may be attributed, is in like manner rendered a very improbable explanation, in consequence of the isolation of the crest between two very deep valleys of ice, and the proximity of the summits of all this group of mountains. At a league distant from this place, below Zermatt, near the bridge over which the road passes, from the left to the right bank, we again find, on granitic gneiss, the same polished surfaces, furrowed and striated, and those rounded prominences which Saussure calls *moutonnées*.

On the southern slope of the Alps we have observed the same phenomenon, both in the immediate proximity of the glaciers, and at continually increasing distances from them, to which they cannot reach in the existing physical conditions. On the route from Cormayeur to the Col de la Seigne, for example, we find one of these surfaces opposite the glacier of the Brenva, which may, however, even in the present times, advance that length, and shut in the Allée-Blanche, as happened within our recollection with the glaciers of the valleys of Bagne and Saass. But this supposition cannot be admitted in regard to the polished and striated rocks which we have noticed between Macugnaga and Pesterena; nor in regard to those of Val-Quarrazza, at the northern base of Monte-Turlo; and still less in relation to the distinctly marked ones which we observe near St Vincent in the vale of Aosta, on issuing from the defile of Mont Jovet.

What must we, then, infer from the series of the same phenomena continued, with little interruption, from the bottom of our glaciers, to such distances as that from St Vincent to Mont Cervin, or rather from the lake of Bienne to the glaciers of Savoy? Shall we be obliged, by assigning the same causes to the same effects, to believe that the former extension of our glaciers was much beyond the most advanced limits they now attain; or rather that a covering of ice had enveloped the whole of the terrestrial globe, if it be true that the same phenomenon likewise appears in Sweden, England, and other countries very remote from high mountains? It must be confessed that this inference, drawn from a single series of facts, acquires great weight by the consideration, that it is likewise the same which MM. Venetz and Charpentier have deduced from the examination of ancient moraines, and by the facility with which, by adopting it, we solve some of the most difficult questions relating to the diluvial era. But it must not, at the same time, be concealed, that, by adopting this explanation of these facts, we place ourselves in opposition to what seem the best established opinions respecting animal and vegetable life before and during the diluvial period, and to all that physics and astronomy have taught us regarding the laws which regulate the temperature of our climate, and of the terrestrial globe in general.

Among the different facts relative to the diluvium observed during my journey of this summer, there is still one which appears to me deserving of the greatest attention from geologists. This is a large bank or range of hills which separates the basin of Ivrea from that of Biella, extending to the foot of the Alps, over a length of five or six leagues, as far as Santja, in the plain of Piedmont. The height of this range, on the road from Mongrande to Bolengo, cannot be estimated at less than a thousand feet above the neighbouring plain; it then diminishes in height as it recedes from the ridges of the Alps, in such a manner that the latter, when seen at a distance, appear like a very uniform talus, which had been cut on its two sides by the rivers which it separates. The declivity on the side of Ivrea is steep, and the road therefore descends in zigzags; it is here also that the elevation of the bank or range is greatest. The back of the hill is rendered very unequal by longitudinal valleys, which seem to have been produced by erosion, and an hour is spent in crossing from one declivity to the other. The entire mass of this hill appears to be composed of Alpine debris. Blocks of gneiss, and other rocks of all sizes, many of them from 15 to 20 feet in length, with their edges little worn, are scattered over the whole surface, and wherever the soil itself is disclosed, nothing is to be seen but unstratified gravel and sand, inclosing a great number of these same blocks. If we traverse the plain of Ivrea, and ascend Lessolo to the plateau of Vico and Brosso, we perceive a like number of alpine blocks covering the whole of this rather steep declivity, which rises to a height nearly equal to that of the opposite edge above the soil of the plain. It is rather remarkable, that, in the same plain, this transported deposit seems to have completely disappeared, and that the fixed rocks, syenite and limestone, shew themselves in numerous places. Nor is it less surprising that, at a very short distance from this ancient theatre of the violence and overflowing of diluvial waters, we find on the east, between Biella and Masserano, and on the west, near Castellamonte, tertiary hills composed of a partly moveable and partly argillaceous deposit, yielding to the action of the smallest rivulets, and inclosing subapennine fossils of the most delicate structure.

De Saussure, after having described these hills and alpine detritus, adds the following reflections: "Do not these two hills which converge under an angle of about 100°, at the entrance of the vale of Aosta, evidently mark out the margins of a current which became widened on issuing from this valley? I know not whether I am giving way to an illusion, but it appears to me, that, in the want of the testimony of ocular witnesses, we cannot imagine monuments more forcibly proving the truth of a fact." Notwithstanding the entire conviction of our celebrated geologist, the founder of the theory of glaciers, I am almost disposed to believe that the partisans of new doctrines respecting moveable rocks or boulders, will see in these hills a classical example of the colossal grandeur of ancient moraines, and that, if they happen to examine the moutonnées and striated surfaces in the neighbourhood of Ivrée they will declare their conviction that the ancient glacier, of which we discovered traces at St Vincent, had extended as far as the edge of the great plain of Piedmont.*

Description of an Improvement on Rutherford's Registering Thermometer. By Mr John Dunn, Optician, Curator to the Society of Arts.† Communicated by the Society of Arts.

The maximum and minimum registering thermometers of Rutherford are not only the simplest, but by far the best yet invented; indeed, all that is required in their construction, beyond an ordinary mercurial thermometer for the maximum, and a spirit of wine one for the minimum, is, to place them horizontal, and introduce into each a small index, in the one to be pushed up by the mercury, and to be dragged down by the alcohol in the other. No difficulty has been felt in executing the minimum one so as to *act* with certainty; this is not the case, however, with the mercurial one, for it has been found that the glass enamel index used by Rutherford himself is drawn back by the mercury, and the same happens with various substances. The material usually employed, and which answers best, is steel; this, however, is often rendered useless by the mercury amalgamating with it. Various fluids have been introduced to get the better of this, but all of these are liable to the objection of mixing with the mercury. After various unsatisfactory efforts, I at last found that, like the story of Columbus and the egg, it was the easiest thing in the world, for it so happens, that, although mercury attracts glass and amalgamates with steel, there is (for our purpose) no attraction between glass and steel, and mercury does not amalgamate with glass; it is therefore only necessary to introduce or interpose betwixt the mercury and steel a small piece of glass, or second index, as is done in the thermometer on the table.

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* *Bulletin de la Société Geologique de France* for February 1840.

† Read before the Society of Arts for Scotland, 15th January 1840.

On the Glaciers which anciently covered the southern side of the Mountain-chain of the Vosges. By M. Renoir. 75

I have long remarked, at the foot of the *ballon* of Alsace, on an arkose formation called the *Tête-de-Planches*, which overlooks the town of Giromagny, a great number of transported blocks of stone, some of the accumulations of which have a volume of 125 or 128 metres, and the height above the present level of the valley amounts to 107 metres.

I was accustomed to regard these blocks as having been transported by some great currents; this was the theory most generally received; but Captain Le Blanc, after having heard the debates which took place respecting glaciers, at the meeting of the Geological Society at Porrentruy, in September 1838, was the first, as far as I am aware, who considered the blocks of Giromagny as possibly belonging to the ancient moraine of a glacier.

This view of them did not correspond to my notions, and as M. Le Blanc had not at that period seen any moraines, his opinion could not modify mine; and it was almost with regret that, in the month of July last, I made a first examination round the *ballon*, and in the valley of St Amarin. This examination somewhat modified my former notions, and gave rise to some uncertainty, which made me feel the necessity of studying existing glaciers more particularly. The Society will permit me to enter into a few details in this place, in order to shew the process through which I went to establish my convictions.

We now know, from what has been pointed out by Messrs Venetz, Charpentier, and Agassiz,* that the marks which glaciers leave behind them as they retire, are, 1*st*, *Terminal moraines*, composed of sand, gravel, pebbles, and even at times a great number of boulders, the whole more or less rolled, forming banks and curved lines throughout the whole width of the valley, whose concavity is turned upwards, higher towards the middle than towards the extremities, of a triangular form, and having the exterior face generally more inclined or steeper than the interior. 2*dly*, *Lateral moraines*, nearly of the same composition as the former, but arranged in longitudinal mounds, deposited on the two flanks of the valley at the same height, two by two, following all the contours of the windings, entering every sinuosity, and having an inclination which represents that of the surface of the glacier. 3*dly*, *Median moraines*, resulting from the junction of the lateral moraines of two glaciers uniting. These moraines present the form of a triangular prism, whose axis is in the direction of the valley, and the nearer its middle, the nearer the size of the glaciers approaches to equality. 4*thly*, Whenever the nature of the rocks permits, the bottom and sides of the valley exhibit perfectly polished surfaces, together with particular indentations, in the direction of the glacier's motion; also *striæ*, or fine parallel lines, likewise running in the same direction, that is to say, in the direction of the valley's inclination, but never following the greatest inclination of its sides. These striæ are particularly characteristic. *Lastly*, Large blocks *not rolled*, often resting, as in equilibrio, on one of their smallest faces, and forming lines, &c. more or less extensive, on the sides and bottom of the valleys.

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* Many of the memoirs illustrative of this subject have appeared in the previous numbers of this Journal.—EDIT.

These marks I had to re-examine more carefully than I had done at a period when they were less known, and when less importance was attached to them. I therefore traversed, during last September, a part of the glaciers of the Bernese Alps, that of the Rhone, &c. I endeavoured, more particularly, to become acquainted with the character of moraines, from those which our glaciers yet deposit, to those from which they have for a long period retired. I examined with the greatest attention, the rocks polished by the motion of ice over them, as well as the parallelism and general direction of the fine striæ which are to be seen almost everywhere. I have found these polished stones above the glaciers, at heights greatly superior to the surfaces of the latter, which would seem to prove that they have been much more considerable than they are in the present day. I have likewise observed that, even when taking into account the nature of the rocks, the polished surfaces are more numerous and more extensive in the higher parts of the mountains than in the valleys, an appearance contrary to what would be produced by attributing the polishing of these rocks to water, since, in the latter case, it would be most frequent and perfect when the pressure would be greatest, that is to say, in the depth of the valleys. It is impossible not to perceive, as was done by the philosophers who first observed them, that the surfaces are better polished, and the striæ more distinctly preserved, the nearer we approach the glaciers, a circumstance which seems to prove that they have been more recently left by the latter than those in the low parts of the valleys; which, again, is the reverse of what would have happened with currents of water.

Immediately above Ober-Gestelen, are to be seen the remains, still perfectly recognisable, of a terminal moraine; the village itself is built on another of greater extent, and better preservation. I think I discovered at Viesch the remains of a median moraine, which must have been formed by the union of two lateral moraines, which belonged, the one to the glacier of Viesch, the other probably to the glacier of the Rhone, although this point is seven leagues distant from the latter. We likewise notice the remains of terminal moraines immediately above Sierre.

With regard to lateral moraines, I met with them less frequently than the others, owing, no doubt, as M. Agassiz says, to their usually being much above the heights which we commonly ascend to. No one, however, can fail to remark the one to be seen between Lavey and Morcle. It is, as it were, stratified, and the strata, as well as the blocks of stone, are inclined to the side of the mountain, because the exterior side of this moraine alone remains. M. de Charpentier attributes this kind of stratification to the water which existed between the glacier and the mountain. I am indebted to this distinguished savant for the most valuable information. I had the advantage to visit, along with him, some of the lateral moraines which lie in the neighbourhood of the salt district of Devans; they are very recognisable, and even well preserved in many places; the prevailing nature of the blocks which partly compose them, is the conglomerate of Valorsine; we likewise notice calcareous blocks brought down from the neighbourhood of Martigny.

I likewise visited, as pointed out to me by M. de Charpentier, the numerous and beautiful transported stones collected to the north-west of the town of Mouthey. The blocks, many of which are of the cubic dimensions of 1300 and 1400 metres, have proved to me, in the object of my researches, as a ray of light, for never could such masses be transported to the height where they are now found by a current, however great and impetuous it may be supposed to have been. Besides, a considerable number of the stones are resting, as if in equilibrio, on one of their small faces; others, and these of the largest size, are supported by two or three points at most, and in such precarious positions, that it is difficult to conceive how they can continue permanently in such a state. They must then have been deposited quietly and slowly, and are evidently the remains of an ancient lateral moraine.

As to polished rocks, I have traced them in the valley of Hasli, and in that of the Rhone, from its origin to near Bex. I have uniformly observed, that they lose something of their polish, and that the striæ disappear more and more in proportion as they recede from the origin of the glaciers.

I took all possible care not to confound the ancient moraines with mounds formed by water and the deposits produced by avalanches; and I have settled in the conviction, that glaciers, of much greater extent and force than any now existing, formerly occupied the valleys of the Alps throughout their whole extent. This first conviction was of the greatest value to me, and the observations I had made, and the knowledge I had acquired, were indispensable for the solution of the question I had charged myself with. I asked myself whether, after having ascertained the former existence of glaciers at the foot of the Alps, at only four hundred and a few odd feet of absolute height, it would be surprising again to find traces of them at the same height at the foot of the Vosges, and, *a fortiori*, at the height of 1250 metres on the *ballon* of Alsace? The recollection of what I had seen among these mountains, would perhaps be sufficient to establish a comparison between their appearances and those I had observed in the Alps, and thus determine my opinion; but, in order to do this with more certainty, I now return to the consideration of the former of these two chains.

The valley of St Amarin, entered by Thann, presented me with nothing remarkable till reaching the village of Moosch, where are to be seen a great number of granitic blocks, disposed on the summit, and on the west and south-west sides, of a mountain of transition-slate, known by the name of *La Tête*, at the bottom of which the road passes. Some of these blocks, resting on one of their small faces, appear to have been deposited tranquilly. They have been little rolled, and it may be observed, that they are generally deposited in the direction of the great Valley.

Higher up, we come to Wesserling, built on an extensive deposit composed of sand, pebbles, and large blocks more or less rolled. This deposit has the form and position of a terminal moraine, if we pay attention to the axis of the upper part of this valley; but, as it extends to some distance like a table-cloth, it might likewise be regarded as a bank or deposit formed by water, the more so as it occurs at a point where the valley becomes greatly widened; however, no trace of stratification is here visible. What has determined my opinion in favour of a moraine, independently of the particular disposition of the large blocks and angular fragments, is the discovery of beautiful polished surfaces with their striæ, on the right bank of the rivulet of *Thur*, at about a kilometer above the great buildings of Wesserling. I ought, however, to add, that, with the exception of these surfaces, which are of small extent, and which have escaped the notice of those who preceded me, because they were not searching for them, this deposit, and that of which I am about to speak, are the most equivocal of all that I have met with.

At the opening of the transverse valley which descends from the Col de Bussang into that of St Amarin, we meet with a new collection of stones, which may likewise be taken for a bank, but which seems to us, from its form, and on account of the polished surfaces of which we shall afterwards speak, to be, with more propriety, regarded as the lateral moraine of a glacier which descended from this transverse valley, proceeding from the surrounding summits, such as Drumont, Tête de Perche, or even perhaps from the *ballon* of Giromagny, which, by uniting to that of the glacier of the great valley, descending, probably, from the great Veutron or the high summits of the other side, would have formed a median moraine; very short, it is true, because the two glaciers united immediately, as is proved by a terminal moraine to be seen a little further down, imperfectly preserved, but recognizable at its two extremities.

Continuing to ascend the valley, we soon find, above the village of Oderen, the remains of a new moraine, disturbed by waters, but still well characterised. But it is immediately below the village of Gruth, or rather, at the commencement of the village, that a beautiful terminal moraine is to be seen in a state of good preservation. It must have been a powerful one; for, notwithstanding the great sinking which the melting of the ice it necessarily contained in its interior, as is the case with all other moraines, must have caused, it is still of considerable height. The centre of the village is likewise situated on another moraine parallel to the first; but the form of it is less obvious. Lastly, at the north-west point of the rock which bears the ruins of the Chateau de Wildenstein, are still to be seen the remnants of a fourth.

One of the reasons which led me to consider these accumulations of stones as moraines, is, that their absolute height is greater in the middle than towards the rocks which encompass the valley, as is the case with all the moraines of existing glaciers. The cause of this peculiar form is known, and has been stated by the savans who have occupied themselves with glaciers; a form peculiar to moraines, and which is the very opposite to that which deposits formed by rapid currents would assume. Moreover, polished rocks appear along the whole right bank of the rivulet Thur, at heights more or less considerable, whenever the rocks have been sufficiently hard to preserve their polish, or when they have been somewhat protected by their position from the action of atmospheric agents. It is true that this polish is no longer perfect, and has, consequently, been incapable of preserving the striæ; but we have noticed, that the same deterioration is observable not far from glaciers, when the rocks are not very hard, or when they have for a long time been left by the glaciers. We see nothing of them on the south-west declivity, because, being there more exposed to the action of the atmosphere, the surface is entirely decomposed, covered with debris, and in a state ready to slip downwards in numerous places.

To the south of the villages of Oderen and Fellering, and at a great height, are to be seen a considerable number of blocks of all sizes, a little scattered on the declivity of the mountain, many of the largest of which appear to support themselves by a few points only on others of smaller dimensions. I regret much that time did not allow me to study their state and position nearer.

From the bottom of the valley of St Amarin, I returned to the lateral valley of which I have already spoken, which leads to that of the Moselle by the Col de Bussang. I immediately perceived, above the village of Orbey, near the road, several polished surfaces, of small extent it is true, and not so well preserved as those of Wesserling; but their small extent is of no importance, since these vestiges, as we have already stated, are not necessarily any thing else than the remains of broad surfaces almost entirely destroyed. Higher up, near the point where the road turns abruptly, I again found similar surfaces, better preserved, and their striæ quite visible at upwards of 500 metres above Wesserling.

From this point, as far as the village of St Maurice in the valley of the Moselle, and even as far as the summit of the height of Alsace, I met with nothing characteristic, unless the debris, more or less rolled, and in unstratified masses, intersected in different directions by ravines, and covered with some blocks which encumber the valley of Bussang to St Maurice, be not the remains of a long moraine, which rested on the south-east declivity of the small chain of *Tête des Corbeaux*, opposite to that of *Tête de Perche*, or of the height from which the glacier may have descended.

It is in descending the southern declivity of the height of Giromagny, and towards the bottom of the declivity, that the proofs of the ancient existence of a glacier become evident. A little below the *Saut de la Truite*, we begin to discover, even in the ditch by the roadside, the first traces of a polished surface, with well preserved striæ. A little farther down, these same surfaces appear well developed, and extend throughout the whole breadth of the valley, as far as the outlet of the gorge which incloses the road; but they are not so well preserved, and, doubtless for that reason, exhibit no striæ. They are seen even on the rocks of the left bank of the Savoureuse, at a greater height than that of the rocks of Tête des Planches, that is to say, at upwards of 130 metres above the valley of Giromagny, on all the places sufficiently hard and sheltered against this action of the atmosphere to have remained entire, and not fallen into an earthy detritus.

But it is here in particular—that is to say, from the outer side of the gorge as far as Giromagny—that the moraines shew themselves in the best preserved and most unequivocal manner; as much so indeed as perhaps could be expected when we enumerate all the causes which have tended, and still tend, to deform and destroy them,—such as the melting of the ice which they contained at their formation, currents of water, cultivation, &c.

A first terminal moraine first shews itself; it is the worst preserved and least important, it is broken in many places, and a fixed rock found in its interior, may afford a pretext for doubt; but three beautiful terminal moraines of considerable size, nearly quite parallel and conformable to the general type of modern moraines, make their appearance in the valley, like three undeniable witnesses, before arriving at the village of Puy; that is to say, extending over a space of half a league. And lastly, the northern part of Giromagny, at about 400 metres from the tower, is likewise built on a great terminal moraine, interrupted by the bed of the Savoureuse and by the cut of the road, and which, in spite of these, as well as building and cultivation, is still in good preservation.

Now I think I can afford an explanation of the manner of transportation and the precarious position on one of their small faces, not only of the boulders deposited on the mountain which overlooks Giromagny, but likewise of all those to be seen on the two declivities of this part of the valley. These blocks are the remains of two lateral moraines, of which that on the right has been the most considerable. Taken together, they form at the same height, two lines inclined towards the plain as the surface of a glacier must have done, penetrating into the sinuosities to be seen near Puy, precisely as we observe in actual moraines. If these blocks had been transported by currents, they could not have penetrated into these anfractuosities, when they are surmounted by heights transverse to the current, and on which they would necessarily have been deposited. It may be useful to observe, that the blocks of Tête-des-planches, which are the largest and most numerous, are placed in relation to the valley of Giromagny as those of

Mouthey in the Valais, of which I have spoken, and which certainly belong to ancient glaciers, are placed relatively to that of the Rhone : that is in regard to the upper part of the valley at the point where it turns. The same observation applies to those to be seen above Fellering and Oderen.

Time and the nature of my engagements did not allow me to explore again the part of the beautiful valley of the Moselle which extends from St Maurice to Epinal and beyond it. Situate below the heights of Giromagny and Servance, I doubt not but that it presents fine and numerous traces of extensive glaciers, which it has for so long a time no doubt contained on its sides. I have traversed it twice, but almost exclusively occupied with the study of crystalline rocks, I would not afford sufficient attention to orographical appearances of the nature of those which now occupy us; so that my recollection does not enable me to come to any conclusion regarding it. I purpose next spring to study it in this new point of view, and to give the result of my observations in the notice which will accompany the topographical and geognostic chart of the environs of Belfort, which I hope to be able to publish forthwith. Meanwhile, what we find concerning this valley under the title of *groupe des blocs erratiques,* in the work of M. H. Hogard on the Mountains of the Vosges,* allows us to discern, especially if provided with M. Rozet's good topographical and geognostic chart, the remains of terminal moraines in those accumulations of stones in ancient lakes, and lateral moraines in the deposits of pebbles and blocks lying at different heights on the flanks of the valley, and even on the sides of the neighbouring mountains, *and which run in the direction of the valley, and stop suddenly instead of descending to the lower declivities which are near them.* This judicious observer rightly judges that the hypothesis of the transportation of blocks of stone by currents was inadmissible, for he says, in the article on deposits in elevated places, p. 194 :—" Besides sands, rolled pebbles, and fragments of rock, composing these transported deposits, of which I have spoken, we likewise find blocks of large dimensions, sometimes exceeding twenty cubic metres, *the transportation of which, in the places where they are found, cannot be explained by means of the same forces which have conveyed the others.*" Thus, in order to explain the great height at which these collections are found, M. Hogard has recourse to another hypothesis,—that of a change of level in the different parts of the valley, subsequently to the transportation of these debris.

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* Description mineralogique et geologique des regions granitique et arénacée du système des Vosges. Epinal, 1837.

The strongest consideration indicative of the origin of all these remains, is derived from considering them collectively. In fact, if we see in our valleys only collections of stones which we ascribe to moraines, we might, perhaps, in conformity to one of the old systems, regard them as the remains of the banks of ancient lakes. But in regard to these huge transported blocks, deposited at great heights on the declivities of mountains, their transportation by means of water and muddy currents, would always remain incomprehensible; for we cannot cite as an example, even on a small scale, the last breaking up of the Dent-du-Midi in the Valais. In that case, the blocks only descended in obedience to the law of gravitation, aided merely by a muddy current, and in reality they surmounted no obstacle nor ascended again; on the contrary, the residuum and debris were extended in the manner of avalanches, without any resemblance to the forms of a moraine. But do we here find only the forms of a moraine and transported blocks? Shall we regard these lateral banks, all of them inclined, and often considerably so, as having contained the waters of lakes? Can the latter ever have inclined surfaces? Shall we say that they have been deposited by currents having this inclination? In such a case the currents must have been very rapid, and could not have formed, in the midst of their course, heaps of stones corresponding to the bottom of the valley. Lastly, these round surfaces, small as well as large, polished on the faces which must have been opposed to the currents, quite as perfectly, as on those which would have been directly exposed to them; equally worn on every part of the same heterogeneous rock, without any difference resulting from solubility in water, hardness, the presence of crystals, fossils, &c., and presenting all the characters of rocks which are still polished every day by our glaciers; in particular, fine and parallel striæ, constantly running in the direction of the general movement, and similar to what diamond points fixed together in a large frame, would trace upon polished marble; striæ which could not therefore be traced otherwise than by angular fragments of hard rock, fixed in a solid body having a regulated movement, such as takes place in the mass of a glacier. And let it not be said that these surfaces have been worn by the friction of blocks in their passage, for in that case they would not rise into nipple-like prominences, often forming a hemisphere of rather small diameter. Let it not moreover, be said, that the striæ have been engraved by the hard angular points which often project from the surface of these blocks; in such a case, they could not exhibit the sustained direction now observed, since a block rubbing upon a rock and driven forward by a violent current, rolls upon itself, or continually turns upon the rubbing surface, by continually changing its direction. When, therefore, we see so many proofs accumulated in the same point, as in the valley of Giromagny, it is impossible to resist conviction, and there is scarcely anything save a geometrical demonstration, that could be attended with greater weight.

A proof of another kind is derived from the state of the soil at the bottom of the two declivities of the chain. M. Henry Hogard, observing that no debris of calcareous rocks was found in the ancient alluvium which covers the soil at the bottom of the north declivity, remarks:

" The current which has carried along the blocks of which I speak (*boulders*), has followed the direction from SE. to NW.; *it ran parallel to the general direction of the valleys of the Vosges*, in which the deposits of ancient alluvium have been made by the movements of the waters which flowed, as they do now, towards the north-west. Of this fact we may be convinced by studying *the nature of the deposits, and of the materials composing them.*"

" Thus we see the rocks of the high regions descend towards the plain, and *the debris of calcareous rocks are never found ascending.*"

Now, I had myself long since remarked, that the ancient alluvium or *terrain de comblement* of the southern declivity, which descends the mountains, following likewise the slopes of the valleys in the directions north, south, north-west, and south-east, is exclusively composed of the debris of the superior rocks, without ever presenting specimens of calcareous rocks before reaching the level of the latter. From this I am likewise entitled to conclude, that these debris have been carried along *by a current coming from the north-west parallel to the general direction of our valleys, since no calcareous fragment has ascended.*

Here, then, are two currents directly opposite to each other, descending from the same point of the Vosges, the source of which we must consider as having been at the summit, a supposition incomprehensible, and altogether inadmissible.

All these difficulties disappear, and every thing admits of a natural explanation, if we place on the heights (ballons) of Giromagny and Servance, and on the neighbouring elevations, glaciers, which, by their continual movement, would transport, without effort, all the fragments of rocks detached from the summits, and the melting of which would furnish, for a long period, torrents and powerful rivers, conveying to a distance from both sides of the ridge of the chain all the substances which we now call ancient alluvium.

All the other great valleys of the Vosges affording grounds for similar observations, it is easy to perceive that in these mountains, as in the Alps, boulders and rolled pebbles extend in a fan-shaped form all round the system, which could never be the effect of a current.

Thus, then, as appears to me, the existence of ancient glaciers on the Vosges mountains is established, and yet we know that the height of Giromagny, the culminating point of those we are considering, does not exceed 1250 metres.

If this phenomenon of cooling belongs to the earth, has its temperature, at one time, been capable of sinking to such a degree? Or have the valleys, at some period or other, changed their level, and done so without entirely altering their place? Was the epoch when ice could continue permanent at about 400 metres of absolute height, between St Maurice in the Valais and Bex, the lowest point at which I could observe its traces among the Alps, the same as that when glaciers descended to the same level among the Vosges at Wesserling and Giromagny?

Now, we know that, oftener than once, upwards of fifty spots have been counted on the sun's disc, many of which have been calculated to occupy a surface quadruple the extent of that of our globe, and continued for many years. It is on record that about the year 535 the light of the sun was *diminished* during fourteen months, and that in 625 the *half* of the disk was obscured for a whole summer. There is no reason to regard this last spot as a maximum; could the sun, by enveloping himself entirely in a sombre veil, at one time, have plunged us and our planets into thick darkness and universal ice?

Finally, in the planetary spaces, the temperature being unequal, like the dispersion of matter, could our sun, in its movement, now known, round a centre with which we are yet unacquainted, have drawn all his system along with him into a colder medium, from which he issued only to be plunged into it again, at determinate epochs, which we may perhaps one day be able to calculate?

It is not in a simple note like the present that we can attempt to discuss such questions as these; besides, M. de Charpentier will soon supply us with reasons which will satisfy all our requirements on the cause, probably accidental, of the formation of large glaciers.

Since M. Agassiz has discovered polished surfaces on the southern declivities of the chain of Jura, which the Society verified at its meeting at Porrentruy, we may believe with him and M. de Charpentier, that glaciers formerly covered all the great Swiss valley; but considering that I cannot find traces of the ancient existence of glaciers in plains remote from mountains, I am led to believe that, at least in our southern parts of Europe, glaciers have never extended much beyond the foot of these chains; that they may have constituted immense masses, but distinct, and generally not continued from one chain to another, and perhaps even from one mountain to another.

With regard to the mode of the transportation of blocks, I believe that if they had slidden, as has been asserted, by their own weight over an inclined and continuous surface of ice from the summits of the Alps, as far, for example, as the first southern slopes of the Jura, the face exposed to the friction in all those of large size would necessarily be polished, but I have never observed this effect in any of them.

If there still issue from beneath our glaciers, reduced as they now are, rivers of which, in the favourable season, many are of great power from their origin, how much greater must those have been which emanated from those immense masses of ice which covered perhaps entire countries, particularly during the melting which reduced them to their present condition, a melting which would be rapid, if the return of heat was sudden. Now, the torrents of our glaciers sometimes carry along with them, from beneath the latter, such considerable quantities of sand, coarse gravel, and even pebbles, that the country at a distance is covered with them; may we not, then, be permitted to ascribe to the great currents which proceeded from the ancient masses those great mixtures of sand and rolled pebbles which still encumber our lower valleys, and which may be traced without interruption to the height of existing glaciers, or the places which have borne ancient ones, without having recourse to the hypothesis of a deluge, the effects of which would be different from what we now witness.

After the secretary of the Society had read the above paper, M. Constant Prevost stated that he had seen on the road to Chambery the surfaces of calcareous rocks deeply furrowed. On these surfaces he had observed pebbles foreign to the country, and in particular a block of greenish slate, which might be fifteen feet in diameter. He was of opinion that these effects might be produced by causes analogous to those just described.

At the request of M. de Roissy, M. Leblanc gave some explanation respecting the character and length of the striæ which have been spoken of above. These short and fine striæ, he said, have been produced by insulated angular stones harder than those on which they have left their marks, in consequence of the double action of friction and pressure. The mamellated parts (*parties mamelonnées*), on the contrary, would result from the friction of the glacier itself, moving on its under side. I may add, continued M. Leblanc, that M. Fergeaud, Professor of Physics to the Faculty of Strasburg, has discovered analogous phenomena in the mountains of the Black Forest in those of the Vosges and Pyrenees, which he has lately examined, and which he also endeavours to explain by the theory of glaciers.

M. Voltz has remarked, in deposits of granular iron-ore, striæ, which were terminated by a grain of that substance.

M. de Roys said, that in the chain of the Alpines, between St Remy and Arles, he has noticed a great number of these polished surfaces looking as if overspread with a varnish, and presenting a few striæ. These hills do not rise beyond 100 or 150 metres above the Mediterranean, and he does not think that this effect can be attributed to glaciers.*

On the Origin of Granite, and on the application of the Huttonian Theory to the present state of Geology. By M. B. STUDER of Berne. In a Letter to Professor Bronn.†

The letter you sent me at the close of last year, contains so much that is calculated to excite, and touches upon so many important geological inquiries, that, throughout the whole winter, I have been anticipating much pleasure in the prospect of answering it. Since, however, my approaching journey to Italy has been determined upon, I have been so much occupied, that I have been unable to command time for the calm consideration of scientific subjects. But for this, I would willingly have supplied for your Journal, not a hurried letter but a well-considered paper, in answer to your doubts concerning the origin of granite; and the more so, as these doubts, in a still greater degree, are entertained by many of our colleagues. The time, in fact, seems to have arrived, when the question of the origin of granite should receive a full consideration, or, at all events, a discussion as to the principles on which the inquiry should be conducted. While then, in the mean time, I renounce every thing like profound research, you are truly welcome to my more cursory thoughts, although at the risk of a modification of my views, or an entire abandonment of them,—after I have heard the objections that shall be offered, and more searching and deliberate inquiry.

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* Bulletin de la Société Geologique de France, for Feb. 1840.

† From Neues Jahrbuch für Mineralogie, &c. Jahrgang 1840.

Although the older notions respecting the origin of granite, and especially of the crystalline siliceous rocks—and which regard them as mechanically formed aggregates, or aqueous deposits, are for ever laid aside, a difference of views may still exist as to the place in which these rocks were formed, and as to their original nature. Thus, it might be supposed that granite was produced by the metamorphosis or fusion of the more ancient deposits, and that its place of formation lay only in the outermost crust of the earth;—or, it may be regarded as the earliest solid crust of the as yet molten globe, and so be the basis of the oldest deposits;—or, as you remark in your letter, it may partake of both of these origins, which, being conceded, we might thus regard the more recent granites, partly as new uprisings of the original and still fluid matter under the general granitic crust, and partly as the transformed product of the sedimentary deposits. But to proceed to your inquiry—whether I acknowledge any difference between original granite and that more recently formed, between granite which forms the basis of the sedimentary deposits, and that produced by transformation, I answer, so far as mineralogical characters are concerned, most decidedly not. I am well aware that there are keen partisans of the theory of metamorphosis who pretend that they can distinguish, by hand specimens alone, between a primitive gneiss or mica-slate, and one which is the product of metamorphosis, and between a primitive and a more recently formed granite. To such skill as this, however, I make no pretensions; and all such discriminations have hitherto invariably appeared to me altogether arbitrary, and the result of far too limited observation.

Turn we therefore from these mineralogical characters, at least till better taught, to geological ones: And here we must, on the one hand, state it as an undisputed and observed fact, that granitic syenites, granitic gneisses, diallage or gabbro-rocks, gabbro-slates, talc and mica-slates, cover for miles sedimentary deposits containing petrifactions; it must also be allowed that there is an intimate union between these rocks and sedimentary rocks, through the medium of gradual transition: these facts allowed, it may then be inquired whether the dogma that granite forms the general basis or ground-work of all formations, and is peculiarly the original rock of the earth, reposes on a very sure or fixed foundation. Probably, we should not entertain any doubt about this conclusion, were we to consult our elementary treatises only, or were we simply to notice upon our elegantly coloured sections, the large red masses on which the name of granite is pre-eminently conspicuous. On more mature reflection, however, this opinion cannot but be regarded as doubtful, nor recognised as any thing more than a mere dogma or article of belief, not an object of actual observation. For, were we to bring forward ever so many examples, in which the deepest deposits discovered in numerous points are not granite but some other rock, still it would be objected that granite may lie beneath; could we demonstrate that most of the minutely examined granitic districts, are evidently of recent formation, as well as the strata which lie immediately beneath and above them, still it might be asserted that these upraised granites prove nothing more than the presence of a deeper lying general granitic mass; and were we to extend this conclusion to all massive rocks, that we might gain a wider field for the action of metamorphosis, then we are called to believe in the unproportionably greater distribution of granite by including all porphyries, diorites, serpentines, and trachytes. All these postulates, however, it will be observed, rest, in the last instance, upon the necessary demand of our reason for a fixed foundation for the first sedimentary deposits, and a sufficiently cooled stony bottom for the primeval ocean; and in what rock, it may be asked, if not in granite, shall we seek for the material of this basis.

The doctrine is grounded upon the philosophy of the past century, which assumed that the foundation of all things, including the sea, had been narrowly and sufficiently examined; that it was as easy to give an account of the history of man, and the organic world, as it was to ascertain the intentions of nature in matters the most insignificant and most important. Far more useful, however, would it be, than many of those sections which geologists are in the custom of hanging about their rooms, were they supplied with a representation of a large circle, exhibiting, exactly in due proportions, the semidiameter of the earth, with the extent of its crust so far as it is accessible to us. A glance of this plan would be much more instructive to many of us, than the display of the coloured profile of the MM. Webster and Nöggerath; and it would go far to overthrow the belief of many who imagine that we have succeeded in penetrating, through the diversified surface, to the primitive mass beneath. If geologists are content to be ignorant of the primitive condition of the globe, and to recognise nothing but chaos anterior to the oldest sedimentary formations, then the above-mentioned facts remain the only supporting points of our explanations. When we see the common *Fucoid* slate and *Macigno* sandstone, passing into chlorite-slate, serpentine, and gabbro, we shall, till additional observation teaches us otherwise, assign the same kind of origin to all serpentines and gabbros. And when, in other places, the same strata change into mica-slates and gneiss; and in others still, the gneiss becomes converted into gneiss-granite, then must we also regard granite universally as the product of metamorphosis. It by no means surprises us that the most perfect granite is principally to be found at great depths, at the base or nucleus of the crystalline strata: for it is there that the product of the most complete transformation must necessarily occur.

Although I affirm that the metamorphosis of *Flysch* rock into crystalline quartzose rocks is a fact, yet I am far from pretending that I can explain this process. Chemistry has indeed furnished us with some important explanations concerning many hitherto enigmatical appearances, but it is not yet in a condition to solve the higher problems of geology; and observation has far outstript theory. To deny facts for any such reason as this, however, as is actually done, is to imitate those who rejected the laws of Kepler, before Newton had deduced them from gravitation. The Newton of Chemistry, I may add, has yet to appear, who, resting upon geological observations, has to demonstrate the higher principles upon which the construction (*Stöchiologie*) of the System of the Alps is regulated. All endeavours of this kind have hitherto been unsuccessful, and the cause of failure may in part be discovered. For, supposing that our elements could be reduced to still simpler ingredients—and no chemist will assert the contrary (?)—and that under the pressure of the whole weight of the sea, and at temperatures which lie far beyond the limits with which we are acquainted, forces were in action, of which we can scarcely form an idea, as certainly existed at the time when our mountains were raised from the depths of the ocean to the regions of eternal snow,—then, to subject these conditions, and all the appearances and processes which accompanied this great event, to the laws of our chemistry, could have a result similar only to our exposition of the planetary movements, in the times of Galileo, when it was sought for solely in the empirical laws of gravitation. Besides, we are placed here in far more disadvantageous circumstances, for the mighty processes now under consideration do not pass along the sphere of our observation, as do the phenomena of the heavenly bodies, but become known to us only in the traces they leave behind them.

The formation of crystalline rocks from sedimentary deposits, has been compared to the changes which are produced by trap-dykes in the neighbouring rocks, or by the smelting fire on the walls of furnaces; and hence it has been supposed that every transformed mountain-mass must have been in contact either with apparent or concealed plutonic rocks. It is quite possible that high temperatures may have essentially contributed to the process under review; but, in other respects, the adduced comparison appears exceedingly defective; and chiefly, as it has often enough been stated, because the influence of the dykes, extending at most but a few feet, bears no proportion to the metamorphosis of a whole mountain. Besides, our physical and chemical theories are in most cases satisfactory in the explanation of the observed influence, whilst the metamorphosis of the *Flysch*-rock into gneiss or serpentine is based merely upon hypothesis alone. But the complete difference of the two processes is made evident by the simple observation, that in so far as the crystalline silicates have been produced from sedimentary deposits, we cannot ascribe the metamorphosis to their influence, that is to say, to their own production. There is still another fact, hitherto little insisted on, which appears to denote a very striking difference between the two appearances; for it seems quite incompatible with this theory of contact, that in the Alps, at all events, the change has peculiarly manifested itself in the external and higher masses, whilst the inner and deeper strata are nearly unchanged,—the rocks in which the metamorphosis is most conspicuous, are entirely separated from the alleged sources of the change by great masses more than 1000 feet thick, and without our being able to perceive any internal or concealed uprising of massive rocks in the form of veins. Thus, in the Southern Alps, dolomite forms the highest rocky precipices; and it is only after passing over a long range of stratified deposits which contain fossils, that we descend to the red or black porphyries. And how striking is it that in those places where the different kinds of rocks are in juxtaposition, as at *Predazzo*, the limestone passes not into dolomite, but into granular marble; exactly as in the Grisons, where the limestone lying close upon serpentine, or surrounded by it, is always free from magnesia, though often white and transparent, like a product of contiguity: at a somewhat greater distance, the same limestone forms immense exposed masses of dolomite. In the Grisons and *Glarus*, the common *Fucoid* slates prevail as the principal underlying rocks, crystalline strata of hornblende, syenite, and gabbro overlying it. In the *Bernese Oberland*, the uppermost portions of the older Jura limestone have been converted into marble, cipolin, and talc-slate; and, at a greater distance from the gneiss of the Alps, and separated from it by the system of the nummulite limestone, which is 10,000 feet thick, and by the *Niesen* chain, we find likewise the upper masses of the newer Jura limestone, in the *Simmen Thal* and the *Saane Thal*, converted, sometimes into a calcareous cipolin, sometimes into red or green iron-shot slate; among these slates there occurs a light-coloured limestone, which is several hundred feet in thickness, unstratified, fissured in all directions, and of a scaly granular structure; and it is only deeper still that we find the common black limestone, compact, distinctly stratified, and destitute of any organic remains. We should hence be led almost to suppose that electro-polar processes had been exerting their agency in the upper and external parts of the mountain, whilst, contemporaneously, the deepest foundations had been subjected to the influence of agents acting from beneath; and we can see no reason why such opposing agencies should be confined solely to the Alps.

In connection with the puzzling appearances of the metamorphosis of whole mountains, the facts which have hitherto been regarded as principally supporting the upraising theory can take only a subordinate place. The formative act of the Alpine system is also a phenomenon which originally differs essentially from all the volcanic processes with which we are acquainted, and with which the foregoing facts are allied, but is probably calculated to diffuse light upon these processes themselves. When we see sedimentary deposits in the interior of the Alps passing into serpentine, which, farther on, forms dykes, and thus overflows, lava-like, and produces contact phenomena, why are we not entitled to assume that, in other regions where we can follow the dyke masses into a common rocky trunk, the connection of this trunk with the original sedimentary deposit remains hid from us, because it is only in the Alps that the internal laboratory appears to have been broken open? When, in the Grisons and in the *Simmen Thal*, we see limestones, which, at one extremity, shew themselves in undisturbed stratification, and at the other, split into many layers, or passing into *breccias* and *conglomerates*, and as such swelling into high mountain-masses, or extensive irregular masses, the suggestion arises, that, in consequence of some subterraneous conglomerate formation, there may take place a swelling and upraising of the surface resting upon it, followed by an explosion, and that the pent-up debris is thrown out. In this stœchiological convulsion of rocks, it may well be supposed there is a much greater increase of volume than in one which is merely mechanical; and in this, perhaps, we find the cause of the very marked extent of the mica-slate and gneiss formations,—the great height to which they rise,—their including the neighbouring limestones, and also the dislocation of these latter over the *molasse*. It would thus be much more simple to recognise the cause of the outpouring of the lava in the pressure of the walls upon the rocks which had become fluid, than, with Cordier, to suppose, that the whole of the crust of the earth is put into action, for the purpose of forcing out a portion of the still fluid central mass of the globe. Such a tremendous machinery could not fail to produce results, in comparison of which our mightiest lava streams dwindle into utter insignificance.

Respecting a theory which, like the one of metamorphosis, promises to assume a conspicuous place in science, it becomes us to inquire into its origin, and to trace its previous development.

The principle of metamorphosis was first, it would appear, at the end of the last century, suggested by Hutton, and with the full merit of originality. It is necessarily involved in the proposition that all stratified rocks are not primitive, but have proceeded from the destruction of older rocks;—that they have thus become more or less consolidated by the internal heat of the globe, and have thereby been transformed into masses possessing a crystalline and slaty appearance. Hutton, moreover, regarded granite and trap as substances which had issued from the interior of the earth in a liquid state; and he felt embarrassed by their sometimes assuming the appearance of stratification. His opponents, also, it should be noted, have particularly dwelt upon the close connection between granite and gneiss, and have properly shewn how inconsistent it was with nature to assign an entirely different origin to the two rocks. Playfair, long ago, answered this objection in words we might still use (see paragraph 146 of the Illustrations), by assuming that in stratified granite, as in gneiss, the lines of stratal separation in the original sedimentary deposit of the beds have still been preserved, whilst the mass itself underwent a metamorphosis by crystallization, whereas in masses of unstratified granite these traces are wanting. Playfair, moreover, believed, that granite itself had its origin from older sedimentary deposits.

After the re-opening of literary intercourse with Britain, these views spread upon the Continent, chiefly by means of the French translation of "THE ILLUSTRATIONS," which appeared in the year 1815,—of the work of Boué in 1818, and of Necker in 1821, both of whom studied geology carefully in Edinburgh under Professor Jameson, and also of MacCulloch's work, and the Transactions of the Geological Society. They found a general acceptance, however, only after the dominion of the Wernerian geology was completely overthrown by Von Buch's celebrated treatise on the southern Tyrol, and when the original Plutonic principle of the theory of rocks, originating in Italy, was again restored to its proper place. In the same work, Von Buch has made a more decided and most interesting application of the principle of metamorphosis, and, at the same time, has essentially extended it, as he has done the theory of elevation itself, by the important agency he assigns to sublimation, and the power of vapours.

On one occasion, I well remember, as M. Merian and I were wandering in the *Glarner* Alps, in the summer of 1826, the origin of gneiss and mica-slate from sedimentary rocks was frequently the subject of our conversation; and in a short notice which I gave of that journey in the *Zeitschrift für Mineralogie*, our opinion as to the correctness of these views is clearly declared. In that notice, the metamorphosis of secondary slates into mica-slate and gneiss, was, perhaps for the first time suggested, not as a theoretical view, but as an ascertained fact; in the same way as Von Buch had previously proved the transformation, in nature itself, of limestone into dolomite. In our tour through Glarus, we happened to pass into a part of the mountains where this metamorphosis is exhibited in a more striking way, perhaps, than in any other part of Europe,—where, on the extended sides of the mountain, the eye can, at a glance, follow the whole of the changes, from the greyish-black of the *Glarus*-slate, through violet and purple to the brightest red; and from the light grey of the common limestone to the brightest straw-yellow of the dolomite,—where, on the *Kärpfstock*, even a still higher degree of transformation appears, as we find the strata have now become particoloured and shining, containing quartz masses, which include druses of rock-crystal, felspar, garnet, and hornblende,—until, finally, we find ourselves surrounded with rocks such as we observe at St Gothard and in Chamounix. This is so much the more remarkable, because it was in the black slate quarries of Matt, at the foot of these mountains, that the slate containing fossil fishes was first observed as the predominant rock of the bottom of the valley, and also because, on the southern declivity, as we descend from the *Kärpfstock*, to the *Eind* valley, the common *Flysh* soon again presents itself in great thickness among the variegated rocks. I shall here take the liberty of quoting the following sentence of a letter which M. Elie de Beaumont did me the honour of addressing to me in August 1838, with the purpose of adducing it as an invaluable testimony as to the correctness of our views; " I have particularly examined," writes the distinguished French geologist, " both during the past year and the present, the environs of *Spitzmeilen* and *Murtschenstock*, and, with much pleasure, have recognised all that you described some twelve or fifteen years ago, in a memoir which I requested the editor of the *Annales des Sciences* to translate and publish in French. You have there signalized one of the most curious and most evident instances of metamorphosis which the Alps present, and, at the same time, one of those which most decidedly proves that these phenomena are of recent origin in the Alps, since a part of the rocks which exhibit them rest upon the *nummulitic* system." Now that the theory of elevation has become so universally popular, the principle of metamorphosis, which is so closely connected with it, finds many supporters, especially among those geologists who have had frequent opportunities of studying the slaty crystalline rocks. Keilhau, in the years 1836 and 1837, pointed out many remarkable and extraordinary facts observed in the neighbourhood of Christiania, which likewise go far beyond the reach of the present state of chemistry; although they refer more to the consequences of immediate contact than to those changes which are produced upon the great scale. It, moreover, would perhaps have been desirable that Keilhau should not have been so minute in his explanations, because the learned world is very apt to throw away, with a theory which it may deem untenable, the facts which are intended to elucidate that theory. The views, also, which M. Elie de Beaumont advanced in 1828, regarding the metamorphosis of the secondary rocks of the *Tarentaise* into *cipolin-limestones* and crystalline slates, have produced a very considerable effect.* As in the Alps, so in the Pyrenees, the principle of metamorphosis was to M. Dufrenoy a strong point of support in the explanation of the most important relations and now scarcely a year passes over our heads, during which, in mountains and districts which had not previously been examined, or, at all events, in relation to this point, new support to our theory is not procured. In Italy, the practical geologists Sismonda, Pareto, Guidoni, and Savi, have become converts to the new belief; and the last-named individual, in his latest writings, shews himself much inclined to suppose that serpentine and gabbro were derived from macigno. With what energy Hoffman has adopted these views is evident from all he has written, and especially from his works upon Italy. Through his means the *Apuanian* Alps have become classical for the study of metamorphosis.

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* From that time especially has the spell been broken, which, from the earliest date, induced the belief that the highest Alps were to be considered as the first-born of the earth's formations; and, with this notion, has the opinion vanished, that, from the appearance of a rock itself, the epoch of its formation may be determined.

It is rather surprising that these views, which have not been put in a peculiarly strong light in the writings of Hutton and his followers, should have spread so widely, whilst another principle of Hutton's doctrine, notwithstanding its greater simplicity, finds much difficulty in obtaining the consideration it merits. I allude to the fundamental law that geology cannot scan the origin of things, but can observe only the succession of transformations which has arisen from Neptunian and Plutonic influences, and that there is not to be found in the earth's crust within our reach any thing that is primitive,—any original rocks, but only newly formed, or transformed fragmentary ones; a principle which, after so many and such important discoveries, we may still class with the most considerable, as it comprehends the change from organic into inorganic matter, and that of the latter again into the former. The great talent with which Mr Lyell has demonstrated the doctrine of periodic change in regard to the nature of the earth's surface, and the general approbation bestowed on his work, have not been able, even in Britain, to induce many who are zealous advocates of metamorphosis, to abandon the old path; and still less has the principle extended over the Continent. For this unfortunate result, a great part of the blame is, I think, due to the expositions of geology which are supplied in books, and by lectures. Although nearly the whole ground upon which Werner based his system has been demonstrated to be untenable, still his footsteps are pursued in relation to geology; and this science, as it has long done, maintains the character of a history of the globe. Whether we proceed with Werner, from the oldest formations to the newest, or, as is now generally done, reverse the procedure, the historical representation always requires a beginning and an end as epochs of time, and one is opposed to the idea of a chapter on tertiary and secondary formations without a concluding chapter on primitive rocks. What, however, shall we say if, sooner or later, this science, which at present is regarded as so important, and which disputes the palm even with sublime astronomy,—if geology or geognosy should cease to be enumerated among the natural sciences ? However grievous the prospect must be for the geological courses of my learned colleagues and myself, still I fear that the mournful consummation cannot, in the long run, be prevented.

If we more narrowly examine the right and title which geology possesses to the field which has hitherto been assigned to it, we find in the enumeration, first of all, a constantly increasing succession of formations, enumerated like so many different portions of the earth's surface. According to the unanimous opinion, the species of rocks are of very inferior importance among the characteristics of these formations ; and those rocks of the Wernerian school, which are still taken into consideration, must continue to decrease in proportion as the formations lose their local character, by more extensive research. On the other hand, the doctrine of the earth's formation appears more clearly distinct, in proportion as it is divested of all extrinsic considerations, among which I reckon whatever relates partly to the rock itself; that is to say, in so far as is not closely connected with the organic characters, and partly to all the changes and transformations to which it has been subjected ; and in proportion to our endeavours to comprehend and exhibit the formations as they may have been originally constituted. But, thus regarded, the doctrine of the earth's formation comes necessarily to be included in the province of organic natural history, and becomes organic geography, whose province is the consideration, not only of the present world as now constituted, but also of the previous and the earliest developments of the organism. Whatever occurs in these inquiries regarding kinds of rocks, reduces itself to the sandy and clayey formations which we see originating in our moors, lakes, and seas, and which neither require great mineralogical knowledge nor expensive collections. A work like the *Lethaea*, but giving somewhat more prominence to the organic formations of a more general kind, such as coral-reefs, infusoria, peat, and so forth, and still more to the local description of those regions which have become classical from their particular formations, as for example, the Parisian basin for the lower tertiary, Thuringia for the *Zechstein* group, &c., would better correspond with what is desiderated, than those disquisitions which abound in geological books, whose authors not being zoologists, occupy themselves chiefly with descriptions of the mountain rocks, and the relations of stratification, and thereby confound every thing. What remains after the separation from our science of the doctrine of the earth's formation, has assumed, in our latest works, more and more the aspect of physical geography, and must indeed altogether coincide with this science. To treat separately, of the effects of erosion, and of the doctrine of sedimentary deposits, as something independent of all that is necessarily associated with this subject in physical geography, is altogether unscientific. The effects of heat and pressure in producing the consolidation of these deposits, and in inducing peculiarity of structure, different coloration, and so forth,—how they, by stronger influence, produced metamorphosis, or partial fusion; the formation of mountain-chains from these deposits, and their elevation from the bottom of the ocean,—the outpouring of the molten masses in dykes and streams—all these agencies of the new theory can be satisfactorily described only in accordance with the principles of general physics, and are entirely incongruous with historical-organic-geography; and, in these matters, mineralogical skill will find ample scope for its due exercise.

And now this letter, which should have been a short one, has exceeded all bounds. This day four weeks, I start directly over Mont Cenis for Turin, whence I proceed to Genoa, and other places.

BERN, *March* 2. 1840.

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Physical and Chemical Examination of Three Inflammable Gases which are evolved in Coal-Mines. By Dr GUSTAV BISCHOF, Professor of Chemistry in the University of Bonn. Communicated by the Author.

The financial department of the Prussian ministry commissioned me to investigate the inflammable gas which forms in the Prussian coal-mines what is termed Fire-damp, and to make experiments in explosive mixtures with Davy's safety-lamp. Chemists, from investigations made in England, regard this inflammable gas merely as carburetted hydrogen, and even, after marsh gas, as the purest carburetted hydrogen gas that can be obtained; but the analyses of the gases from the English coal-mines by no means justify the assumption, that the inflammable gases from other coal mines are invariably of the same nature.

It is but rarely that a good opportunity presents itself in mines, of collecting this gas in a perfectly pure condition. If the mines in which it is evolved be damp, its evolution is rendered evident by a peculiar sound which may be exactly compared to the noise caused by the movement of a number of crabs in a basket. We also perceive, on the floor or the roof of the galleries, bubbles, which, by bursting, cause this noise, and which can often be ignited by the lamp. Should, however, the mines be perfectly dry, the evolution of this gas can only be ascertained by the formation of fire-damp. Besides these evolutions, extending over a greater or smaller extent, by means of which the fire damp is produced at such places, when no strong current of air exists, there are also particular points where the inflammable gas flows out in greater or less abundance, from fissures in the coal-strata or neighbouring rock. Such places, which are termed blowers *(blaser)*, are, however, but rare in our mines. It is evident that it is such blowers alone which admit of the collection of a perfectly pure gas.

In the year 1837, while visiting some of the coal-mines in the vicinity of Saarbrücken, I found a gaseous exhalation of this description issuing from a fissure in sandstone of the coal-formation in *Gerhard's stollen.* By inserting in the fissure a tube, and luting it with clay, the evolved gas could be ignited, and it burned with a blue flame three or four inches in height. The flame was so perfectly blue and without a trace of yellow, that, when it was compared with the bluish-yellow flame of the inflammable marsh gas, one was inclined to regard the evolved gas rather as carbonic oxide gas than as carburetted hydrogen. I endeavoured, by means of a small pneumatic trough, to collect some of it; but it was not possible, as the gas could not overcome the pressure of the smallest column of water. Having become aware of this circumstance, I turned my attention to other means in order to be able, on my second visit to *Gerhard's Stollen* in the autumn of 1838, to collect the desired quantity.

Physical Relations of the Inflammable Gaseous Exhalations in the Coal-pits of Saarbrücken.

I have already described the mode in which the inflammable gas issues forth in *Gerhard's Stollen* in *Louisenthal.* When, in the year 1838, I visited the place for the second time, a tube was inserted with greater care than before, and the fissure, as far as it could be followed up, was spread over with clay. The gas now burned with a flame which was twelve to fifteen inches in height, but which was not as formerly of a pure blue colour, but was only blue beneath, and yellow above. I shall afterwards shew that the same inflammable pit-gas can burn with different coloured flames under different circumstances.

If we allow a continuous stream of the gas to be directed on the tongue or into the nose, we can neither remark a disagreeable taste nor smell. It was tasteless, and without odour. The miners told me that they had never noticed a peculiar smell in fire-damp, but they had experienced a pressure on the eyes and the temples. This induced me to allow the gas to flow into my eyes for some minutes. I experienced, however, nothing peculiar in the eyes; but others, who repeated this experiment, said that they felt a peculiar sensation in the eyes or temples.

The high flame of the gas, as it issued either directly from the fissure, or from the tube, which was five lines in diameter, could, with facility, be extinguished. It was extinguished by blowing upon it at a distance of from three to six feet, or by making a movement in the air with the hand. This appears less remarkable when we take into consideration, that the gas flows out with no higher pressure than that of the atmospheric air. The slightest current of air, therefore, is sufficient to blow out the burning gas. It is different, as is well known, when inflammable gases are evolved from apparatus intended for the formation of gases, and when they issue out under a greater or less degree of pressure. In order to extinguish such a current of gas, a current must be employed which is greater than that of the gas itself. It is further known, that this current must be so much the greater in proportion as the inflammable gas evolves heat during its ignition, and as it is more easily inflamed. Hence, of all the inflammable gases, hydrogen is the most difficult to extinguish. The heat evolved by the flame of the burning pit-gas is inconsiderable; for one can extinguish it by slowly closing the tube with the finger, without burning himself.

The current of gas can neither be set on fire by lighted tinder nor by a lighted cigar, even when we attempt to kindle it by strongly blowing with the mouth. If we do not blow, both are speedily extinguished by the current of gas.

The temperature of the current of gas was ascertained by me to be 55°.7 F., by placing, for some time, the bulb of a delicate thermometer in the gas. The same thermometer indicated a temperature of 54°.7 F. in a bore in the rock near the fissure, which was eight inches deep; as deduced from three observations made in the morning, at noon, and in the evening. It being assumed that the gas brings with it the true temperature of the place where it is originally evolved, that the mean temperature of the ground of the outer crust of the earth at Saarbrücken is 49°.5 F., and that the increase of temperature towards the interior of the earth amounts to 1° F.,* for fifty-one Parisian feet, it would result that the gas comes from a depth of 322 feet. The place where the gas issues forth is 210 feet under the surface of the earth; and hence it would appear that the gas comes from a depth of 112 feet under the gallery. It may however, with probability, be assumed, that the gas on its passage is exposed to refrigerating influences, inasmuch as it passes through colder strata and meets with colder water; hence the original temperature of the gas may have been higher than 55°.7 F.; and the current may have come from a greater depth.

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* The place where the gas comes out, is of course, at a depth where no changes of temperature occur. However, the communication by the air

A second blower occurs in an old deserted mine near *Wellesweiler*, about twenty English miles from Saarbrücken. It was reached about forty or fifty years ago, and since 1816–17 has been included in a copper funnel with a prolonged pipe. It occurs in a principal fissure in slate-clay at the bottom of the gallery. At some distance from the blower, a bore has been made for 119 feet under the floor of the mine, and a stratum of coal from seventy to eighty inches thick has been found at a depth of forty-five feet.

I found this current of gas still surrounded by the above-mentioned funnel, which I employed for the collecting of the gas. This current is much smaller in quantity than that in *Gerhard's Stollen*, for the flame of the gas was only 2 to 3 inches high. But the gas was just as devoid of smell and taste, and burned like the other, with a flame that was yellow above and blue beneath.

The temperature of the gas was 54°.6 F., and that of the rock 51°.6 F. The above-mentioned data being assumed, the gas must have come from a depth of at least 155 feet. The same remarks are applicable to this case as to the other, and hence it is likely that the depth is greater than I have just stated.

I have already mentioned, that the gas in *Gerhard's Stollen* cannot be collected in the usual manner, in a pneumatic trough. The apparatus which I employed for the purpose,

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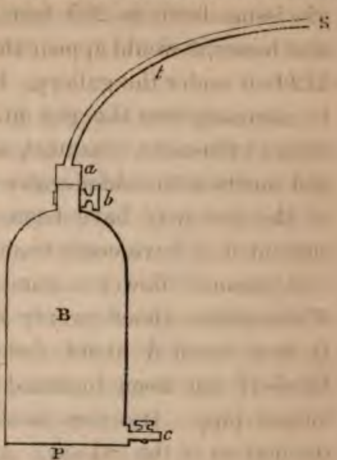
between the galleries and the external atmosphere doubtless causes the stone, to the depth of several feet, to partake of the changes of temperature of the external atmosphere. On the 30th September, when I made the above observations, the temperature of the surface of the earth was some degrees above the mean. The observed temperature of the rock (54°.7 F.), therefore, was also of course above the mean.

consisted of a large bell-glass B, which was closed below with a plate luted to it P, and was provided with a stopcock *c*. A second stopcock *b* was luted to the bell-glass above, into which a hollow pipe (*Hülse*) *a*, to which a leaden tube was luted, could be screwed. The tube *t* was luted into the fissure of the rock, and after the bell-glass was filled with water, the current of gas was absorbed. I found that, even when the cock was entirely open, not nearly so much water flowed out, or so much gas was absorbed, as there issued gas from the fissure. It was easy to become convinced of this by lighting the gas which had been again forced out from the cock *b*, by means of insertion in a vessel filled with water; for the flame was then only 2 inches high. As this issuing out of the gas took place in nearly the same time as the flowing in, while the gas coming direct from the fissure burned with a flame of 12 or 15 inches in height, only a small portion could be absorbed. I was thus well assured that no atmospheric air had been absorbed along with the gas.



With the assistance of two sets of apparatus, by means of which the gas could be alternately absorbed and again forced out by insertion in water, fifty ordinary wine bottles of it could be filled in one day, which, being carefully closed, were transported to Bonn, secured in vessels by means of water.

In the mine of *Wellesweiler*, these pieces of apparatus for col- [illegible] the gas were not necessary. The circumstance that this [illegible] volv[illegible]m the floor of the mine, which was covered [illegible]h of several inches, gave me good ground [illegible]ust flow out with a pressure greater than [illegible]e. I surrounded the funnel with a layer [illegible]s deep, conducted the leaden tube luted [illegible]tic trough filled with water, and found that [illegible]overcome the pressure of a column of water [illegible] three or four minutes, an ordinary bottle was filled with gas. I took advantage of the favourable opportunity, and filled a hundred bottles, and in the same manner transported them to Bonn, well closed, and secured by water.

It thus appears that the difference between this stream of gas and that of *Gerhard's Stollen* is, that the former issues with a pressure which is somewhat greater than that of the atmospheric air, while the pressure of the latter is just equal to that of the atmosphere. This difference can be easily explained. While the gas from the Wellesweiler mine is evolved from the floor covered with water, all the fissures which descend from this level, and which communicate with the above-mentioned principal fissure, must be filled with water. Hence, all the gas which ascends from beneath is hemmed in by water. This chief fissure, which is probably variously ramified beneath with other gas canals, presents the smallest amount of hindrance to the evolution of the gas, because, in a wide fissure, gas and water can be more easily separated than in a narrow one. Hence, the gas is evolved from this fissure only, and the remaining narrower canals are interrupted by water. In *Gerhard's Stollen*, on the contrary, the current of gas is evolved about 7 feet above the floor of the mine. Hence, also, when up to that floor, all the gas canals are filled with water, this can cause no hindrance, inasmuch as the fissure is probably continued to the surface and is ramified.* An interruption, therefore, which meets the current of gas flowing outwards, will cause it to remove to another place. It is possible that, during continued wet weather, where the fissures communicating with the surface are obstructed by water, the gas exerts a pressure superior to that of the atmospheric air. Such relations, at least, are presented by the exhalations of carbonic acid gas, of which several examples are known to me in the neighbourhood of the *Laacher See*, and of the volcanic Eifel, both of which districts are so rich in very large gaseous evolutions.

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* This supposition has been completely confirmed. An officer of the mine informs me, that the blower has ceased to flow in; doubtless because a stratum of coal has lately been worked 28 feet above the said blower.

As the blower in the mine of Wellesweiler affords a bottle of gas in three minutes, the current thus amounts to about 18 Parisian cubic feet in twenty-four hours. I found that one volume of this gas still formed an explosive mixture with 15 to 16 volumes of atmospheric air. In twenty-four hours, therefore, such a mixture of 306 cubic feet can be formed, which would occupy a length of 14 feet. Should, therefore, such an issue of gas continue for months, without the inflammable gas being removed by means of a current of air, we may well imagine what powerful effects the explosion of so considerable an amount of fire-damp can produce. Notwithstanding the considerable evolution of gas, which, in *Gerhard's* mine, judging from the size of the flame, amounts to at least twenty times as much as in the Wellesweiler mine, yet both mines can be visited with the usual miner's lamp without the least danger. One cannot even perceive the presence of the inflammable gas in the miner's lamp. Hence, we see that, by means of a strong current of air, which exists in both mines, the most considerable evolutions of inflammable gas can be made completely harmless.

On account of its inconsiderable specific gravity, the inflammable gas is always collected at the top of the gallery, and there forms the fire-damp. Hence, in general, the miner can visit without dread, places which are filled with weak fire-damp, with the ordinary miner's lamp, if he keep the same near the floor ; and it is only when he approaches it to the upper part that an explosion ensues. This is only the case, however, in places where either there is no movement of the air, or only a feeble one ; for, in an atmosphere which is in a state of movement, the pit gas is either entirely removed, or so mixed with the atmospheric air, that the whole space is filled with fire-damp.

The ascent of the inflammable gas to the higher portions of a mine, is turned to account in the working of beds, in order to protect the miners from the frightful effects of fire-damp. Thus, should an inclined stratum, proceeding from a principal gallery, be worked from below upwards, and should there be an evolution of inflammable gas, an accumulation takes place gradually at the working place, and the mining must be suspended.

If, on the contrary, the mining operations should be carried on from the opposite side, proceeding from another gallery, and continued from above downwards, the inflammable gas removes from the working place, and the miners can without difficulty perfect their work to the previously abandoned point. If the strata should be very much inclined, so that the mining operations are carried on through such highly inclined shafts, there is no fire-damp to contend with, because, in such a case, the inflammable gas escapes through these shafts, immediately after its evolution.

I may be allowed to take this opportunity of communicating some observations on exhalations of carbonic acid gas, which present the same phenomena, but only in the opposite sense. When carbonic acid gas is evolved in recesses, or in inclosed spaces, which do not at all, or but slightly, partake of the external movements of the air, this heavier gas is collected, and the entering such places is attended with danger to life. The celebrated *Grotto del cane*, the gas grotto (*Dunsthöhle*) near *Pyrmont*, and several similar places in the vicinity of the *Laacher See*, and in the volcanic *Eifel*, present like phenomena. When, however, such exhalations of carbonic acid gas take place in flat situations, and even in much larger quantity, the gas becomes distributed through the atmosphere as soon as it is evolved, and the rapidity of its diffusion hardly admits of the presence of this mephitic gas being made known by its smell. Thus, there is a spot at the village of Wehr, about five English miles from the *Laacher See*, where carbonic acid gas is evolved in immeasurable quantity from hundreds of mineral springs, close to one another, and where, at many points, bubbles as large as the head scatter the water to a height of more than a foot, and where, in the middle of the marsh, the smell of the gas is hardly perceptible.

Chemical Analysis of the Pit-gas from the Wellesweiler Mine.

§ 1. *Examination for Oxygen Gas.*

Nitric oxide gas mixed with the pit-gas did not produce the slightest yellow colour. By adding 54 volumes of nitric oxide gas to 100 volu[illegible] pit-gas, the mixture amounted to

According to the first experiment,	153.6	volumes.
... ... second ...	154.0	...
... ... third ...	153.1	...
Mean . . .	153.6	...

If this inconsiderable absorption be entirely ascribed to oxygen, the latter would amount to only about 0.002 volume. It is evident, however, that this slight absorption proceeds chiefly from the water through which the nitric oxide gas passed. Hence I believe that the pit-gas contains no oxygen, or at least no appreciable quantity. The evolution of this gas must therefore take place without any co-operation of the atmospheric air. This likewise followed from the circumstance, that this gas is evolved with a pressure greater than that of the atmospheric air. When agitated for twenty minutes with a solution of sulphuret of potassium, the absorption amounted to 0.058 volume. But it is plain that the solution had absorbed a portion of the pit-gas. Th. de Saussure, at least, had previously remarked, that inflammable gases are absorbed in considerable quantity by sulphuret of potassium. It was on that account that I was obliged to employ the very imperfect agency of nitric oxide gas for determining quantitatively the oxygen.

§ 2. *Examination for carbonic acid gas.*

The pit-gas renders lime-water turbid. The absorption by means of caustic potash amounted to from 0.041 to 0.043 volume. The original quantity of carbonic acid gas, however, must undoubtedly have been much greater, as the gas in its course comes in frequent contact with, and as it is also collected over, water.

§ 3. *Examination for olefiant gas.*

This examination was performed by means of chlorine gas, and in a flask composed of black opaque glass, of the kind called hyalite glass. After the pit-gas had been freed from carbonic acid gas by agitation with liquid potash, it was mixed with chlorine, and, after the mixture of gases had stood for several minutes over the water used for retaining it, the chlorine was again removed by agitation with caustic potash. Another portion of chlorine was tested as to its containing atmospheric air, by means of liquid potash, and this gas was subtracted from the residue. The following is the detail of two experiments.

Pit-gas free from carbonic acid gas,	154. v.	142. v.
Chlorine gas added,	136. v.	137. v.
After washing the gaseous mixture with liquid potash,	156.5 v.	141. v.
136 volumes of chlorine, however, contained of atmospheric air,	8.35	4.36
Hence the true residue of pit-gas after treatment with chlorine was	148.15	136.64
Absorption	5.85	5.36

If we regard the gas absorbed as olefiant gas, its quantity amounts to 0.038 0.038

The original quantity of olefiant gas must have been much greater before the pit-gas came in contact, in a variety of ways, with water in the gas-canals, because it is more abundantly absorbed by water than is carburetted hydrogen gas.

Two other experiments on pit-gas which had been collected a day later in the *Wellesweiler* mine, afforded, by means of chlorine, an absorption of 0.028 and 0.037 volumes.

An examination of pit-gas for olefiant gas by means of superchloride of antimony over mercury, yielded less satisfactory results, and I shall not therefore communicate any of the details.

§ 4. *Pit-gas and chlorine exposed to the light.*

As I do not know that a perfectly pure inflammable pit-gas has ever before been subjected to this experiment, I took advantage of the opportunity afforded me, to make the trial with the almost pure carburetted hydrogen from the Wellesweiler mine.* To 158 volumes of pit-gas, previously washed with liquid potash, were added, over water, about double the volume of chlorine, and exposed for five hours to the diffused day-light. The volume was diminished to 94. After having been agitated with liquid potash, the residue amounted to 75 volumes. This residue burned, as it appeared, with a flame which was more blue and less yellow than the pit-gas which had not been treated with chlorine. The chlorine had, therefore, decomposed only about the half of the pit-gas employed in the experiment.

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* Gay-Lussac and Thenard (Recherches Physico-Chimiques, t. 2, p. 191) have only performed experiments with gas " provenant de la décomposition de l'alcohol, ou d'une huile, à travers un tube rouge de feu, ou enfin avec les gaz inflammables composés qu'on obtient en distillant une substance vegetale ou animale quelconque. Toujours il y a eu d'action instantanée à la lumière solaire, et en même temps qu'il y a eu detonation violente dans ce cas, il y a eu un dépôt *souvent très considerable* de charbon."

Equal volumes of pit-gas and chlorine did not act on each other in the light of the sun; but one volume of pit-gas and two volumes of chlorine detonated over water with a very feeble explosion, by which carbon was deposited, and muriatic acid was produced in white vapours. Sometimes the detonation ensued instantaneously, and in other instances not for some minutes. The detonation seemed to depend chiefly on the purity of the chlorine. If it was rendered impure by atmospheric air, no detonation ensued, and the water merely rose slowly. The detonation also took place more readily when the chlorine had been first admitted into the receiver, because, in that case, the specifically lighter gas was forced to ascend through the chlorine, and thus to become better mixed with it. This was particularly the case in narrow receivers. The detonation was best effected in a cylinder 18 inches high, and 1½ inch wide, when the mixture of gases occupied a space 10 inches high. Whenever it was exposed to the sun's light, an instantaneous explosion was the result. The mixture at first was expanded, white clouds were formed, and then the water ascended, while a large quantity of carbon was deposited. If the sun was only a little obscured by a cloud, there was merely visible a white mist, which descended more especially from a small projection at the closed end of the cylinder. The water, however, did not fall as in the rapid action, but gradually rose while the mist was in the course of being formed. If, even during this action, the cloud was removed from before the sun, and a strong ray of sunshine fell on the cylinder, still no detonation took place. During such a slow action of the two gases on each other, carbon was never deposited. If the mixture was left standing in the light of the sun so long that the chlorine was entirely absorbed by the water, a strong smell of oil of turpentine became perceptible.* The deposited carbon had a peculiarly sharp taste, which had some resemblance to that of mustard. The cylinder in which the carbon was deposited had the smell of chloric ether.

These experiments were performed during some clear days at the beginning of November. On the 6th and 7th of December I endeavoured to repeat them, but in vain. On the 6th there was a thick fog, and although the sun shone brightly and broke through the mist, yet it was not possible to accomplish the decomposition of the pit-gas with deposition of carbon. On the 6th there was less fog, and it became concentrated on the Rhine, but a light vapour in the whole atmosphere so weakened the sun's light, that, although the gaseous mixture sank and afterwards again ascended, yet it was not possible to produce a deposition of carbon, however frequently the attempt was renewed. It is known that a mixture of hydrogen and chlorine gas likewise cannot be decomposed by detonation, when the light of the sun is weakened even by the most slender cloud or by a light vapour.

§ V. *Examination for Carbonic Oxide Gas.*

The strong blue colour of the flame of the pit-gas was in favour of the presence of carbonic oxide gas. I tested for it by potassium. After the pit-gas had been freed from carbonic acid gas by agitation with liquid potash, it was, according to the process of Gay Lussac and Thenard, introduced into a somewhat bent tube filled with mercury, it was dried by means of chloride of calcium, and a portion of potassium was made to ascend. The potassium was then heated to the melting point. It was found, however, after the tube was cooled, that not only was there no diminution of the volume, but even an increase of 0.109 volume.

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* Dr Henry (Annales de Chimie et de Physique, vol. xviii. 1821), likewise remarks, that the smell of the liquid, which is collected on the surface of the glass receiver in which the oil or coal-gas has been mixed with chlorine, indicates the presence of a compound of carburetted hydrogen with chlorine, and of another fluid which seems to have a great resemblance to oil of turpentine. When we take into consideration that pit-gas only requires to give up 0.4 of its hydrogen, in order to be converted into oil of turpentine, we can easily understand the possibility of the production of this combination of carburetted hydrogen by means of the action of chlorine.

As in this increase of the volume a trace of moisture in the mercury might have had a part, in the second experiment I boiled the mercury in the tube, and proceeded after the cooling in the same manner as before. In order to remove the naphtha, the potassium was melted in a narrow glass tube. In this case, likewise, there was an increase of the volume after the heating of the potassium, but it only amounted to 0.036. Two other experiments, with unboiled and boiled mercury, gave the same results.

Hence it seemed to follow that the pit-gas contained no carbonic oxide gas. Further experiments, which I shall afterwards detail, have however proved, that the potassium cannot be employed for the separation and determination of carbonic oxide gas, when the latter is mixed with carburetted hydrogen gas.

The circumstance, that, according to the experiments of M. Despretz,* the oxides of iron, zinc, and tin, are reduced by means of carbonic oxide gas, afforded some, though but little hope, that, perhaps, by employing one of these oxides, a separation might be effected of this gas from the carburetted hydrogen gas. As, by experiments which I shall subsequently describe, I obtained the conviction that the inflammable gas from the Wellesweiler pit, is an almost pure carburetted hydrogen gas, I took occasion to examine the phenomena presented by it in reference to the above-mentioned oxides. I conducted it very slowly through a tube filled with chloride of calcium, and then through a glass tube which was filled with pure oxide of zinc, and was heated to a bright red heat. This tube was, as in the analysis of organic substances, placed in connection with a tube filled with chloride of calcium, and with the well known apparatus of Liebig, for the absorption of carbonic acid. Some water was in fact produced, whose weight amounted to 3.6 grains, and the solution of potash had absorbed 24.05 grains of carbonic acid. According to this result, the composition of the pit-gas would be as follows:—

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* Annales de Chimie et de Physique, t. xliii. p. 222.

Hydrogen,	0.0567
Carbon,	0.9433
	1.0000

But, as we shall see below from the analysis of the pit-gas, only about ⅙th of the hydrogen of the carburetted hydrogen is oxidised, and ⅚ths escape unoxidised. The gas which passed through the solution of potash also burned with a very feeble flame. The carburetted hydrogen gas was then decomposed in the tube, which had been heated, and was filled with oxide of zinc, and the carbon was entirely, while the hydrogen was only partially, oxidised. We thus see that the oxide of zinc is by no means adapted for the separation of the carburetted hydrogen from the carbonic oxide gas.

§ VI. *Effects of Exposure to a red heat on Inflammable Pit-Gas.*

It is well known that chemists generally assume that carburetted hydrogen gas, when it has been repeatedly conducted through a red hot tube, is converted into double the volume of pure hydrogen, and that carbon is deposited. As the carbonic oxide gas suffers no decomposition under the same circumstances, it appeared that the presence of this latter gas, could be most readily ascertained, and its separation most certainly effected, when a mixture of the two gases is conducted through a red hot tube.

I placed a porcelain tube in a furnace, provided it at each end with a tube which was filled with chloride of calcium, and connected these two tubes with two gasometers. In this manner the pit-gas could be always conducted in a dry state through the red hot tube. After a quantity of inflammable pit-gas, previously washed with liquid potash, had been conducted through the apparatus, in order to remove the atmospheric air as much as possible; the porcelain tube was heated to a white heat, the gas was made to pass very slowly through it, and was collected in the opposite gasometer. The gas was then sent back again, and this was continued so long as the volume increased. The volume of the gas was most enlarged by the first passage through the heated porcelain tube, but less by the second and third, and after the fourth not all, however often (10 to 12 times) the process might be repeated.

In these experiments, there was no difficulty in excluding the atmospheric air in the tubes to a minimum,* for it was only necessary to repeat frequently the flowing through of the pit-gas. But it is very difficult to determine directly the volume of the tubes, viz. those tubes which are filled with chloride of calcium. This determination can, however, be made by an indirect method. Thus, if we perform the experiment twice, but with unequal quantities of gas, and if we measure each time the increase of volume, we can thence determine the quantity of gas which was contained in the tubes. Let a and a' be these two portions of gas which have been measured in one of the two gasometers, and let b and b' be the additions to the volumes after the decomposition of the gas in the heated porcelain tube, then, if x indicate the volume of the tubes, and should the increase of volume in the two experiments have taken place under the same circumstances:

$$x + a : b = x + a' : b',$$

consequently . . . . $x = \frac{ba' - b'a}{b' - b}$.

I have performed several experiments, but soon found that the increase of volume had not always taken place in perfectly similar circumstances, whether it was that there had been a variation in the heating of the porcelain tube, or that the carbon remaining behind from previous experiments exercised influence over the decomposition. Thus, I did not in each new experiment take the apparatus to pieces, and remove the deposited carbon, because the volume of the latter was too small to affect that of the porcelain tube in a sensible manner. Out of many experiments, I select two which presented the greatest agreement. The measured volumes, reduced to 28 inches barometer, and 32° F. were the following:

$a = 42.97$ vol. $b = 8.58$
$a' = 43.71$ $b' = 8.69$

Hence $x = 14.75$ vol., and the increase of the volume amounts to 0.149 of the original volume.

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* The presence of the atmospheric air would naturally have caused a partial ignition of the inflammable gas in the heated porcelain tube.

The carbon which was deposited in the porcelain tube consisted of extremely thin foliated particles, which had become so rolled, that they formed thin little tubes, which were from a few lines to an inch in length. They had a steel-grey colour, and a strong metallic lustre, and were perfectly similar to graphite, both as that substance is found in nature, and as it is artificially formed in many iron slags. An empyreumatic smell was perceptible in the porcelain tube.

I ignited the portion of gas obtained from one of the above experiments in the detonation tube. I mixed 1 volume decomposed gas with 3 volumes oxygen, and obtained, in two experiments, the following results:

	Experiment 1. Volumes.	Experiment 2. Volumes.	Mean 3. Volumes.
Gaseous mixture, . .	4	4	4
Disappeared by detonation, .	1.45	1.477	1.4635
Disappeared by use of liquid potash,	0.638	0.613	0.6205

The 0.6205 carbonic acid gas which was produced, corresponds to a similar quantity of carburetted hydrogen gas. Upon the supposition that no other inflammable gas was present except carburetted hydrogen and hydrogen, the latter would amount to

$$\frac{2}{3}.1.4635-\frac{4}{3}.0.6205=0.1484\,\text{v.}^*$$

The gas decomposed by heat thus consisted of undecomposed carburetted hydrogen, ...	0.6205
Hydrogen gas, ...	0.1484
Foreign gas, ...	0.2311
	1.0000

According to what has been assumed 0.1484 v. hydrogen must proceed from the decomposition of the half volume = 0.0742 carburetted hydrogen gas. Upon this principle the increase of volume would amount to 0.11 v. Regarding the foreign gas, there is no other supposition admissible, but that it is partly decomposed atmospheric air. It is true that the analysis which follows below of the pit-gas not decomposed by heat, exhibits only a very inconsiderable amount of a foreign gas; but, if we consider that the gas submitted to the previous experiment remained nearly 24 hours* in contact with a large quantity of water, it must be evident that a portion of the gas must have been absorbed by the water, and, on the other hand, that atmospheric air must have been given out by the water. This must happen so much the more, because the surface of contact between the water and the two gases was great. The atmospheric air which, during the experiment itself, was added to the gas, must naturally cause a partial burning of the pit-gas in the porcelain tube, so that a smaller increase of volume must have taken place than resulted from the above calculation. All these unavoidable circumstances must affect considerably the general accuracy of the numerical results, and hence these can possess but a small value. Besides, the following experiments shew that the decomposition of the inflammable pit-gas is accompanied by other peculiar phenomena.

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* Thus, should a given gaseous mixture consist of x volumes hydrogen gas, and y volumes carburetted hydrogen gas, and if a represent the volume lost by detonation, and b the volume of the carbonic acid gas which is formed, it is easy to perceive that $x=\frac{2}{3}a-\frac{4}{3}b$.

The empyreumatic smell which I had perceived in the porcelain tube after the decomposition of the pit-gas, allowed it to be supposed, that during the process, a peculiar hydro-carburetted compound might have been produced. As in the earlier experiments only small quantities of pit-gas were conducted through the heated porcelain tube, I repeated the experiment with a larger quantity. The apparatus was somewhat altered. With the porcelain tube, which had a much greater diameter (1 inch) than that employed in the previous experiments, I united the previously described apparatus with which I had collected the pit-gas in Gerhard's mine, and I opened the cock so little, that only about a drop of water flowed out in a second. The gas in the gasometer was therefore conducted through the chloride of calcium and the porcelain tube with great slowness. In this manner nearly three bottles of gas were conducted, but without being caused to return as in the former experiments.

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* The experiment itself lasted some hours, and as the gas could not be measured until the whole apparatus had cooled, it was necessary to allow it to stand over night.

After the cooling of the apparatus, I perceived the same empyreumatic smell as in the former experiments, but in greater intensity. It was not unlike that of oil of turpentine or of petroleum. The partially decomposed gas had likewise an empyreumatic smell, quite similar to the products of the dry distillation of wood, or still more of sugar. The gas burned with a feeble bluish flame, which was coloured yellow only at the tip. The carbon which was deposited in the tube possessed the same metallic lustre, and presented the same appearance of rolled up lamellæ, as in the former experiments. Some of these lamellæ had a diameter of 2 lines. I likewise found a dull soot-like powder. The internal surface of the tube itself had a black coating, which adhered very strongly. I attempted to loosen it by means of a feather moistened with alcohol, but I only succeeded with a very small quantity. By filtering the liquid, little portions of carbonaceous dust remained behind, and the filtered alcohol was coloured slightly yellow. Ether seemed to have no effect. Sulphuric acid was rendered somewhat brownish when a glass rod moistened with it was rubbed upon the coating.

It must likewise be remarked, that at the end where the dry pit-gas entered, the coating was brownish-yellow, but in the other parts of the porcelain tube, quite black. Hence it appears that the feebler heating of that portion of the tube gave rise to other products than the stronger heat of the other parts. The little tubes of carbon exhibited a certain consistence. They could be strongly shaken in a glass without being broken, and I had even difficulty in dividing them with a glass rod. On being rubbed, they coloured paper like graphite, and the mark was removed by Indian rubber.

The deposited charcoal was not acted on by nitric acid in the cold. When exposed to a boiling temperature, there appeared to be a feeble action. At least, after the boiling had ceased, the evolution of some gas bubbles could be remarked, which probably consisted of carbonic acid. The metallic glance of the carbon was, however, not destroyed in the acid.

It follows from these experiments, that the carburetted hydrogen is decomposed by heat not merely into hydrogen and carbon, as has been hitherto generally believed, but that, besides, products of dry distillation (solid or liquid carburetted hydrogen compounds) are formed, as in the decomposition of organic substances.

In order to ascertain somewhat more exactly the nature of these carburetted hydrogen compounds, I repeated the preceding experiment with this difference, that between the collecting apparatus and the porcelain tube, the well-known absorbing apparatus of Professor Liebig, filled with concentrated sulphuric acid, was introduced, and was carefully united to both, the whole being made air-tight. Six bottles of pit-gas were conducted through, and the experiment lasted from five to six hours.

After the passage of the gas through the heated porcelain tube had commenced, there appeared in the first ball of the absorbing apparatus, white vapours, which soon rendered the sulphuric acid brown. There then appeared in the glass tube of this apparatus, which was united with the porcelain tube, yellow drops which gradually flowed down into the ball. Afterwards, greenish drops were condensed over the sulphuric acid in the first ball. The acid in the three remaining balls also gradually assumed a brown tint, and, after the termination of the experiment, it became deep brown in colour. I allowed the apparatus to remain at rest until the porcelain tube was entirely cooled.

The decomposed gas burned with the same flame as the former, only with this difference, that the yellow portion was somewhat smaller, and that the flame afforded less light. It thus seems to follow, that the substances, which, in this experiment, were absorbed by the sulphuric acid, but which, in the former, at least partially, remained mixed with the gas in the form of vapour, increased the burning power of the flame. The gas had, moreover, still an empyreumatic smell, which, however, seemed to be feebler than in the preceding experiment. Hence it results that the sulphuric acid did not absorb all the products of the decomposition.

On the following day I separated the apparatus; I was much surprised at finding in the porcelain tube only some trifling little spangles of carbon, whereas, in the former experiment, with a smaller quantity of gas, there was a considerable quantity of carbon. There was likewise a coating, which, however, throughout its whole extent, was browner than in the former experiment. It adhered strongly to the tube, so that it was extremely difficult to get it out. Water did not remove it, but oil of turpentine was a little more effectual. It could only be scoured out with sand, and even then some dark portions remained in the hollows of the porcelain tube.

What can be the reason, that in this experiment so little carbon was deposited? As all the circumstances were the same, with this difference only, that, in the previous experiment, the heated gas could move on without hindrance, whereas in this one it was restrained by a column of sulphuric acid two inches in height, one might be inclined to seek for the cause of the difference of the result, in this difference of circumstances. In fact, by means of the flowing off of the water from the collecting apparatus, the gas between it and the receiving apparatus must always first be so much rarified, as to be able to overcome the pressure of the acid.

The flowing through of the gas can, therefore, take place only by jerks, and it must remain longer exposed to the heat of the tube than in the preceding experiment. But other causes can be adduced for this difference of result.

The greenish little drops of which mention was made above, were partly hardened and partly formed a uniform coating to the ball. It resembled a green-coloured solidified oil. On the sulphuric acid in the first ball, viz., that one turned towards the porcelain tube, a whitish pellicle floated, which, when a movement was made, adhered to the inner surface. This pellicle so entirely covered the acid, that it appeared swimming on it like a solid body. Likewise on the mercury which preserved the gas before it reached the apparatus, and also on the under side of the receiver, there was a white covering.* This pel-

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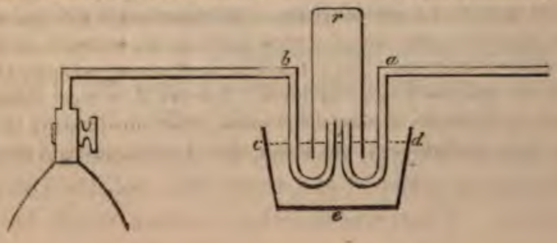
* The absorbing apparatus terminated in a tube *a*; and a similarly bent tube,



licle, which had been deposited from the gas after it had passed through the sulphuric acid, had a peculiar empyreumatic smell.

The sulphuric acid also had a strong empyreumatic odour, which, however, as the acid became diluted with water, was diminished, so that at last only a feeble sweetish smell, as when sulphuric acid and alcohol are united, remained behind.

The diluted acid was neutralized with carbonate of potash. The evolved carbonic acid had also a sweetish smell. The solution had a light brownish colour. After it was filtered, a brown coating remained behind on the filter, which, however, was too inconsiderable in quantity to admit of its examination. The filtered liquid likewise had still a feebly brownish tint.

As the preceding experiments had proved, that peculiar carburetted hydrogen compounds can be produced when inflammable pit-gas is transmitted through a heated porcelain tube, and that these compounds, at least in part, are also absorbed by sulphuric acid, I performed an experiment to ascertain if these combinations are also absorbed by alcohol. I made use of precisely the same apparatus, but filled the absorbing apparatus with alcohol of 92°.5 per cent.

In the first ball, viz., that turned towards the porcelain tube of the absorbing apparatus, there soon appeared, after the gas passed through the heated porcelain tube, white vapours, which were condensed into brownish-yellow small drops. Afterwards there was formed on the cooler side of the ball a yellow covering, in which were perceptible many small yellow drops. The alcohol gradually assumed a wine yellow colour, and the colour increased. In the tube which was fixed in the cork of the porcelain tube, there was condensed a light brown scaly

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b, was united with the collecting apparatus. These two tubes entered a small glass-receiver *r*, in which quicksilver was poured up to *c d*, so that the openings of the tubes were above the level of the metal. I have employed such an arrangement for several years, in all experiments with gases which require an apparatus composed of several parts, as, for example, in the analysis of organic substances. The connection is, of course, perfectly air-tight, i easily made, and easily again removed. I prefer it in most instances to the connection by means of caoutchouc tubes, which must always be proved regarding their air-tightness, and which often remain air-tight for a short time only.

mass, or it seemed rather as if a white sublimate had been coloured of a brownish tint by another substance.

Nothing presented itself on the other side of the absorbing apparatus. It thus appeared as if all products of the decomposition had been absorbed by the alcohol. The hot gas merely caused a little alcohol to evaporate, which was condensed on the mercury.

In the porcelain tube, as in the former experiment, only a little carbon was deposited.

Whether these carburetted hydrogen combinations, which are formed from strongly heated pit-gas, agree with the known carburetted hydrogen combinations which are produced by means of heat from the decomposition of organic substances, or whether they are of a peculiar nature, can only be determined when we are able to obtain them in larger quantity. Such a quantity might be prepared by transmitting a very large quantity of gas through a porcelain tube for several days in succession, and by collecting the products of the decomposition in a receiver. As these products are for the most part condensed directly behind the porcelain tube, it will scarcely be necessary to cool the receiver with ice. It will nevertheless be advisable to select the winter for this experiment, and I hope to devote next winter to this purpose, when I shall have obtained a large quantity of pit-gas.

There can be no doubt that olefiant gas also is not decomposed by heat into hydrogen and carbon merely, as has hitherto been generally supposed, but that also from it, similar products of decomposition are produced. I hope, therefore, to extend my investigations also to that gas.

§ VII. *Attempts to decompose Pit-Gas by Electric Sparks.*

When I performed these experiments, I had not acquired the conviction that no carbonic oxide gas can exist in pit-gas. Since, according to the experiments of Dalton,[*] the volume of carburetted hydrogen gas, when it has been electrified for some time, will be increased to exactly the double, and the whole gas will then consist of pure hydrogen, while carbon is deposited; and since, according to Henry,[*] carbonic oxide gas undergoes no change from 1100 small sparks; it appeared to me, that to electrify the pit-gas, would be a very appropriate method of ascertaining the presence or absence of the latter.

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[*] New System of Chemistry, vol. ii. p. 258.

Pit-gas, which had been purified by washing with a solution of potash, was introduced into a detonation tube over mercury, and dried by means of chloride of calcium. 6200 sparks from a a charged jar were passed through the gas, for which a period of thirty hours was required. Long before this large number of sparks had been transmitted, I could remark no farther increase of volume. Every thing, therefore, had been accomplished which could be attained by this method. One might otherwise easily be deceived respecting the actual increase of volume, when, as is the case during so long a continuance of the electricity, interruptions occur; for the gas expands somewhat in consequence of the heat excited by the sparks, and again contracts during the interruption. Besides, the increase of volume during continued electricity always goes on diminishing, so that towards the end, hundreds of sparks may pass through without a change of volume being perceptible. Unfortunately I could not measure the true increase of volume, as the detonation tube was not quite air-tight, for, during the long continuance of the experiment (four to five days), some atmospheric air gained admittance between the platinum wires and the glass.[†]

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[*] New System of Chemistry, vol. ii. p. 177.

[†] Detonation tubes may be perfectly tight for mercury, and yet not air-tight. I have very frequently found that they were filled with mercury, or that the hermetically sealed end was turned downwards, and that not a drop was allowed to get through for some days; but that, in the opposite position, when standing in the pneumatic trough, after some days some air entered. This air frequently amounted to so little, that it was only after some days that the mercury in the tube fell a few lines. There is no doubt that the pressure of the atmosphere, which in the last case acts on the tube, not only causes this, but that the greater subdivision of the air, in comparison with the mercury, has a share in it. I had previously tried the detonation tube employed in the above experiments, and found that no air entered in twenty-four hours. I can, therefore, make no other supposition, but that, during the shock of so large a number of electric sparks, a space, however small it may have been, was opened between the platinum wires and the glass. I may further remark, that, at the commencement of the experiment, the mercury stood at 12, and at its termination at 8 inches above the external level.

The carbon which was deposited in the tube had a strong smell of oil of turpentine.

One volume of this decomposed gas afforded by the detonation an absorption of 1.1477 volume, and hydrate of potash absorbed 0.3197 volume. The formation of carbonic acid proved distinctly that in the electrified gas there was still present a gaseous compound of carbon. As the analysis has not discovered any carbonic oxide, it is evident that all the carburetted hydrogen gas was not decomposed by the electricity. According to the results of the detonation of the electrified gas with oxygen, its composition was as follows:

Hydrogen, ...	0.339
Carburetted hydrogen gas, . . .	0.320
Foreign gas (nitrogen), ...	0.341
	1.000

Therefore only about ⅓ of the carburetted hydrogen gas had been decomposed. Without doubt, however, a part of the hydrogen gas was burned at the expense of the admitted atmospheric air; for no portion of the carburetted hydrogen gas could be burned, because the electrified gas, previous to its detonation with oxygen, was not diminished by hydrate of potash.

It appeared to me to be of no interest to repeat an experiment that cost so much time and labour, in order to ascertain the relations under which the carburetted hydrogen is decomposed by electricity. It would have been to be apprehended, besides, that a new detonation tube would likewise have been rendered leaky by the electricity. For me the result sufficed, that a complete decomposition of the pit-gas is not possible by this process, and hence that this method is unsatisfactory for ascertaining the presence of carbonic oxide gas in a given inflammable gas.

If electric sparks produce a decomposition of compound inflammable gases by the heat which they evolve, it was to be anticipated that they could effect no perfect decomposition; for the experiments in the preceding section have proved that red heat can also produce no perfect decomposition. In both cases there seems to have been produced a carburetted hydrogen compound resembling oil of turpentine. The formation of such a compound stands, without doubt, in connection with the decomposition of the carburetted hydrogen gas.

Whence, then, does it arise, that the results of my experiment differ so much from those obtained by Dalton? Henry states, that it is only moist carburetted hydrogen gas which can be expanded to double its volume, by means of electric sparks. Afterwards, however, he remarks,* that this is also the case with extremely dry gas. The dissimilarity of results cannot, therefore, be accounted for, from Dalton having probably electrified moist gas, while I operated on perfectly dry gas. I can hardly believe that a more powerful effect can be produced by electricity than by red heat. As I did not at all succeed in completely decomposing pit-gas by means of red heat, although I repeated the experiment so frequently, the results of the English chemist still seem to me somewhat puzzling.

§ VIII. *Phenomena exhibited by Pit-Gas with Sulphuric Acid.*

A bottle of pit-gas was transmitted through a tube 1 foot in length, filled with chloride of calcium, and then through Liebig's absorbing apparatus, which was filled with concentrated sulphuric acid. The acid was not rendered in the least brown, and was not increased in weight. The gas, therefore, contained no non-permanent gas which is absorbed by sulphuric acid. Hence, it follows, that the substance which had rendered the sulphuric acid brown in a former experiment (§ 6.), did not exist originally in the gas, but was only produced after the application of heat.

The absence of a gas capable of being absorbed by sulphuric acid, does not support the opinion that the inflammable pit-gases can be products of a sort of dry distillation.

(To be concluded in next number.)

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* Gilbert's Annalen., vol. ii. p. 194.

† Idem. vol. xxxvi. p. 298.

On the Effects of the Curvature of Railways. By EDWARD SANG, Esq., Civil Engineer, Edinburgh, M. S. A. Communicated by the Society of Arts.*

The prodigious velocity which is now attained on railways, brings out prominently all the defects of their construction, and renders it necessary to attend to every minute circumstance. It is well known that when a railway train is moving upon a curve, there is a tendency to go off the rail, and to continue the rectilineal motion. To prevent the bad effects of this, the outer rail is raised to such a degree as that the line laid across the rails may be perpendicular to the resultant of gravity, and the centrifugal force. This precaution completely removes all tendencyof the waggons to move off the rail, and all pressure against the ends of the axes. The point to which I wish to draw the attention of the Society is the transition from a straight line to a curve, or from one curve to another.

It is considered by some to be sufficient that the straight part of the rail be tangent to the circle which forms the curve; or that the circular parts of the rail have a common tangent at their junction. By this means any sudden angle is avoided; but this is far from sufficient for the exigencies of railway travelling.

To see the nature of the defects of this plan, let us follow a waggon in its course from one direction to another. The instant that it leaves the straight line and comes upon the circle, there is the centrifugal tendency; and there must be a rise in the outer, or a depression in the inner rail. Suddenly the passengers, endeavouring (without being aware of it) to continue moving in a straight line, feel themselves pressed to the one side of the carriage—slightly it may be—but still suddenly. The rise in the rail cannot be instantaneous; and thus, either before or after the change of curvature, there is a want of proper adaptation. Such must necessarily be the result wherever the transition from one degree of curvature to another is sudden.

This inquiry therefore offers itself; What ought to be the nature of the curvature of railways?

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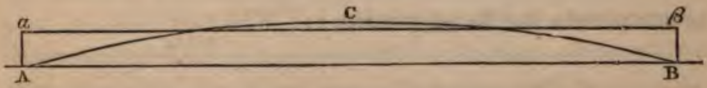
* Read before the Society of Arts for Scotland, 27th May 1840.

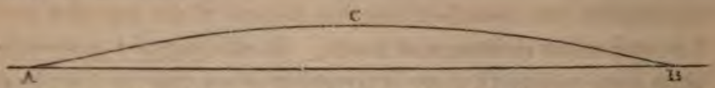
One thing is certain, that the change of curvature must never be abrupt, and that the junction of circular arcs is inadmissible.

Viewing the distance measured along the rail as the absciss, the curvature may be regarded as a function of that distance; and this function must be of such a nature that the curvature may be zero at the point where the deviation from the straight line commences. Putting l for the length reckoned from this point, and ϱ for the curvature, the simplest function which satisfies this condition is $\varrho = nl$. That is, the curvature is proportional to the distance from the said point. But this function is insufficient for the purpose, since it would give a perpetually increasing curvature, while the general object of a railway curve is to lead us from one direction to another—to join two straight parts of the line. For this purpose the curvature must increase, reach a maximum, and again diminish to be zero at the place where the second straight part is reached. The simplest function which possesses the requisite properties for this is $\varrho = nl\ (L - l)$, where L is the whole length from the one straight part to the other. A curve possessing this characteristic would be entirely free from the fault to which I have pointed; yet, pushing our examination still more narrowly, it would not be altogether free from defects, since the vertical projection of the outer rail (supposed stretched in a straight line) would be parabolic.



Thus, A and B representing the two ends of the curved part, and AB the level of the inner rail, the parabola ACB would represent the level of the outer rail; in the case of the circular sweep, the line $\alpha\ \beta$ would represent it with a sudden stop at each end. The curve ACB must undoubtedly be preferable to the line Aα βB (which, in fact, never can be adopted in practice), yet even it must give a harshness at the points A and B; which harshness must augment as the second power of the velocity.

The curve ACB ought to have touched the straight line at the two points A and B, it ought to have presented an appearance of this kind.



Let us retrace the steps of the successive improvements at which I have hinted. First we have the function $\varrho=\frac{1}{r}$ (constant), of which the first derivative is zero. Second, $\varrho=nl$, of which the second derivative is zero; and, thirdly, $\varrho=nl(L-l)$ of which the third derivative is zero; the softness of the action increasing with the number of derivatives. For further improvement, we must take a function of an order still higher. Now, supposing these functions to be completely resoluble into factors, the curve given by them will have as many points of reflexure (or of straightness) as the function has dimensions, and the curve will present a waved appearance. I need not indicate to those who are acquainted with the higher analysis, how the thorough investigation of such curves surpasses the present powers of that powerful science. Such persons will at once recognise in the curve of sines, a transcendental function, having an interminate succession of derivatives, and whose form, easily submitted to calculation, embraces all the essentials of which we are in search.

I therefore submit, as the proper sweep for joining two straight parts of a railway, that portion of the curve of sines which is contained on one side of its axis, and I subjoin a few practical rules for its adaptation.

Notes on the Physical Geography, Geology, and Climate of the Island of Madeira. By JAMES MACAULAY, A.M. and M. D. Communicated by the Author.

Few foreign stations are better known in this country, by name and general description, than the Island of Madeira. Its fruits and wines, and other productions of the soil, have long enjoyed a merited celebrity. The remarkable salubrity of its climate has been established by the number of invalids who have there sought and found relief from disease, and prolongation of life. The boldness of its mountain outline, and the beauty of its landscapes, have from the earliest times been themes of admiration to the navigators of the Atlantic; and no country yet described delights the eye of the traveller with features more striking and lovely. In this paper I propose to note some points that are interesting or peculiar in the physical geography and geology of the island, and advert to those circumstances which determine the felicity of its climate, and the grandeur of its scenery.

Funchal, the chief town of the island, is situated in 32° 38′ 11″ of north latitude, and in 16° 54′ 11″ west of Greenwich; is distant from Cape Cantim, on the coast of Africa, about 360 miles; and is about 240 miles north-east of Teneriffe. In its greatest length the island is 45 miles, and 15 in its greatest breadth; the circumference about 100 miles; with a population of about 115,000, a fourth part of whom reside in Funchal and its adjoining district.

In its general configuration, the island presents the appearance of a range or mass of mountains, of considerable height in its whole extent, above which, in the centre, there rise various peaks and ridges, which mark the water-sheds between the north and south sides of the island. This central mass is everywhere deeply riven by ravines, which are separated from each other by branch ridges running down towards the coasts, and terminating, generally abruptly, in sea-cliffs of immense height. The slope of the land is for the most part more abrupt, and the cliffs are more precipitous, on the north than on the south coast of the island; to which, however, at certain points, the branches of the central mountain reach down without great diminution of their elevation, and form some of the most gigantic sea-cliffs in the world. The headland of Cape Giram, for example, is about 1600 feet in height, and not far from perpendicular.

The loftiest point of the island is the summit of the Pico Ruivo. Concerning the altitude of this mountain there has been much discordance of statement (from 5162, Smith, to 8250, Gourlay), but the authority generally followed has been that of Mr Bowditch, who gives 6164 feet as the result of barometrical observation. Of the various accounts since presented, I consider as most worthy of confidence that of Lieutenant Wilkes, and a party belonging to the U. S. Navy, who visited the island in September 1838. Observations were made at the American Consul's house in the town of Funchal, while the party were on the mountain. They remained on the summit four hours, so that several simultaneous observations were obtained. The result was, 6181 feet above the lower station, which is 56 feet above the level of the sea; or 6237 feet as the height of the Peak. The Torrirhas, the Pico Grande, and several other summits, are within a few hundred feet of the same altitude.

The mountain-range is in most places cut in the most irregular manner, and in general to a great depth, by valleys and ravines, which form the most striking portion of the scenery of the island. The ridges by which the valleys are bounded, are often of the most trifling breadth, standing up like walls of partition between the abysses on either side of them. At one place a road lies across a crest or dike of this sort, which separates the Corral and the Serra d'Agoa, two of the most magnificent valleys of the island.

Although the island is thus in great part deeply intersected by irregular ravines, there are one or two upland plains or table-lands of considerable extent. That called the Paul de Serra, in the west part of the island, is from nine to ten miles in length, and three broad; and its elevation is upwards of 5000 feet. It is a wild, uncultivated, and uninhabited tract, during great part of the year veiled in mist and cloud. The exterior parts are clothed with vaccinium and broom, and other brushwood, but these become rarer in advancing towards the interior and higher ground, and scarcely a moss relieves with its verdure the rocky waste of the Campo Grande. The only objects met with are one or two untenanted huts, built by a benevolent English merchant, as places of shelter for bewildered or benighted travellers.

On the east side of the island is another upland plain, the Serra de San Antonio. Its elevation is in general from 2000 to 3000 feet above the sea. In its aspect it is more like an English heath. On the parts not occupied in cultivation, there is a good turf, and abundance of heath and other indigenous shrubs and trees. Of these the most remarkable are the tree-heath, *Erica arborea*, and a large species of bilberry, the *Vaccinium Maderense*. The Erica grows to an astonishing size, many of the trunks being from five to six, and some eight feet in circumference. In the ground adjoining to the chapel of St Antonio and the pilgrims' house, is a forest of old trees of great size, and many of them upwards of forty feet in height. The stems are covered with the beautiful fern, *Davallia Canariensis*, climbing the trees with its rough creeping root; and various species of rare or interesting flowering plants are found in the neighbourhood. The *Vaccinium Maderense* also grows to a great size, forming beautiful thickets or little woods, in many places from twelve to fifteen feet in height.

The slope of the land, I have said, is more gradual on the south than on the north side of the island. Here the central mountain sends down to the shore lofty ridges, but only at intervals; so that between them are left spaces or basins, in which the land rises to the central heights by a series of gradual slopes, interrupted here and there by ravines and projecting ridges. In a semicircular basin of this kind, bounded by the high ridges that terminate in the lofty sea-cliffs of Cape Giram on the west, and Cape Garajam on the east, lies the district and town of Funchal. The town is situated round the margin, and in the immediate vicinity of a large bay, open to the south; and immediately behind it the land rises gradually, forming terraces of vineyard and garden ground on the sides of the amphitheatre of hills by which the bay is surrounded. The dark and rugged ravines by which these sloping grounds are deeply intersected, stand in strong contrast to the verdure that is interrupted by them. The lower hills are covered with vines, which are here trained over trellis-work of cane, supported by rows of stone pillars. These rows of white pillars, and the beautiful quintas or villas which are scattered on the hill sides, give a fine appearance to the ground above the city. About the upper limit of the vineyard-ground, built on an eminence about 1900 feet above the sea, is the Mount Church, the most conspicuous object in the landscape from the bay of Funchal. Beyond this are forests of chestnut and pine, and other trees; and the scene is bounded by a ridge of mountains nearly 4000 feet in elevation. Between this ridge and the sea, along a great extent of the southern coast, are ranges of low hills and elevations such as those in the neighbourhood of Funchal, and generally under similar cultivation. Among the vineyards are every where interspersed orchards of orange, lemon, almond, and other fruit trees; and a great variety of vegetables are cultivated, chiefly for the supply of the city. Among these are observed the following: *Convolvulus edulis*, the batata, the tuberous roots of which are much used by the peasants; *Allium cepa*, garlic, of which the Madeiranese are great consumers; the churches, and other places of public resort, being always redolent with its odour; *Sechium edule*, choo-choo, a species of gourd, the fruit of which, when boiled, much resembles our "vegetable marrow;" the *Dioscorea*, or *Tamnus edulis*, yam; and many others. The beds of the rivers, and moist places, are covered with an arum, the *Caladium nymphæifolium*, and with the *Arundo donax*, which is tended for the making of trellis-work for the vineyards. There is also some corn, wheat, and barley; and here and there patches of sugar-cane, which was formerly the prevailing produce of the island.*

The difference between the north and south sides of the island is, in every respect, very remarkable. In crossing from one coast to the other, which can be easily done in a day's journey, one seems to be in altogether a different land, so changed is the climate, the vegetation, and the whole aspect of the country. From the city there are two roads to the north of the island, leading to the two villages of St Vincente and St Ann's. In going by one of these and returning by the other, the traveller who has only a few days to spend in the island may see its finest and most interesting scenery. The expedition, however, depends on the state of the weather in the mountains, which are not always passable; and, at any time, few passing visitors who find themselves after a sea voyage thrown for a few days upon a spot of such varied delight as Funchal, are willing to brave the cold, and fatigue, and inconvenience of many kinds, by which the pleasure of viewing the remote scenery must be purchased.

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* The Portuguese chroniclers relate, that, at the time of the discovery of the island, its whole surface was covered with laurels, dragon-trees, cedars, and other forest trees. The first colonists burnt great part of this wood, in order to clear the surface for cultivation. The great Prince Henry of Portugal, whose enterprise and genius in devising and directing maritime discoveries, were excelled only by his sagacity and judgment in applying them to purposes of importance and benefit to his country, immediately on hearing the report of his navigators, projected the introduction of the cultivation of the vine and the sugar-cane. The latter was brought from Sicily, where the Arabs had attempted its culture. There were formerly upwards of a hundred mills in the island; now there is but one, and only a few patches of cane, in the country round Funchal. The vine has long been the chief produce of the island, although, probably, the culture of the sugar-cane will be soon much increased. Of the grain grown in the island, the quantity is not sufficient for more than two or three months' consumption. For flour and corn, the inhabitants depend for supply on North American, Sardinian, and other trading vessels. I left the island in a Greek ship, belonging to the Bay of Nauplia, which had brought a cargo of wheat all the way from the Sea of Azoph. A considerable quantity of milho, or Indian corn, is imported from the Azores. The attention of the public authorities in Funchal is at present much directed to the condition of its agriculture, by which the resources of the island are capable of great improvement.

From Funchal to St Vincente it is about nine hours' ride. For some miles the road lies eastward along the sea-cliffs, commanding in most places a fine view of the sea. The country is here in cultivation; the vine and corn being the principal produce, with occasional gardens of fruit and vegetables for the supply of the city. At the ravine of the Socorridos river, the route leaves the direction of the coast, and proceeds inward by a continuous ascent, often considerably steep. The road being paved and inclosed by loose stone walls, is comparatively uninteresting till the upper limit of the vineyards is passed. The path, now more open and wide, runs across mountain turf and heath, and sometimes through forests of chestnut and other trees, till it passes above the Jardim, the beautiful villa of Mr Veitch, long the British Consul in Madeira. Shortly after this we come to the Corral, one of the finest pieces of Madeira scenery, and which, being within a ride of the city, most strangers are able to visit. Leaving the path we ascend the brow of a hill to the right, and come suddenly upon the edge of a precipice of great height, forming part of the wall of a vast ravine or hollow in the shape of a crater, enclosed almost entirely by lofty mountains, from the rugged sides of which, crags and cliffs jut out into the valley. These cliffs are clothed with forests of laurel and other evergreen trees, nearly to the summits of the mountain sides. On every spot where a projecting ledge, or a fissure in the precipice, permits of their growth, are seen the trunks of aged trees, the dark foliage of which overshadows the gulf beneath. These trees are so far scattered, that the traveller in looking down into the gloomy shade, here and there gets a glimpse far beyond of lighter and less rugged scenery, even down to the cottages, and gardens, and vineyards by the river side in the valley, more than two thousand feet below him.

On returning from this view, we cross the narrow ridge already mentioned, having on the one side the Corral, and on the other side the Serra d'Agoa, also a magnificent valley, more like a ravine in its aspect, and the sides clothed in a similar manner with native wood. The path is only a few feet wide, so that the traveller looks directly down into the immense depth on either side. The grandeur of the prospect from this point of view may be readily imagined.

The north side of the Serra d'Agoa is bounded by the precipitous side of the Pico Grande, a mountain of great height and most imposing grandeur. For some way the path is cut in the face of the cliff, which in some parts so much overhangs the road, that one feels little inclination to pause sufficiently to notice the various wonders of the scenery, or to gather the rare and beautiful flowers and ferns that abound upon the moist rocks in the locality. Amongst these mountain plants, some of the most conspicuous in the spring are the *Ranunculus grandifolius*, Lowe, a fine species with yellow petals larger than those of our Great Spearwort (R. Lingua); *Ruscus hypoglossum ; Asplenium Canariense ; Gymnogramma Lovei.*

After winding round the Pico Grande, the ride continues for several hours through the mountain district; every turn of the path presenting new and diversified combinations of scenery. Having descended from the high grounds, and passed a tract clothed with modern plantations of fir, birch, and underwood, we enter the lovely valley of St Vincente. It lies between two parallel chains of mountains, which run down towards the sea from the central chain of the island. These are, throughout, of great height, and wild and rugged in their form and outline. In some places the valley is much contracted, and at its outlet only a narrow pass is left for the escape of the river. In a more open space formed by the recession of the mountains to the west, immediately before the approximation of the opposite ridges in the sea cliffs which form the outlet of the valley, is situated the parish church and part of the village of St Vincente. The road down the valley runs by the side of the river, or of water-courses derived from it, through a most charming district of garden and orchard ground. The vine is here trained on trees, principally chestnuts, instead of on trellis-work as on the other side of the island. The banana, the cactus, and other plants of the south are absent, but the orange and citron still flourish. A considerable quantity of flax is grown, from which the coarse cloth of the island is manufactured. The gardens present some variety of culture and produce, both as to flowers and vegetables. On the whole, there could hardly be conceived a more lovely spot than this valley. The road, as it runs through the orchard grounds, is overcanopied by the foliage of the vines which cross and interlace in every direction from the upper parts of the lofty trees, forming lines of natural arbour-work to shade and shelter from the sun; while through breaks in the foliage are seen glimpses of the bright blue sky, or the dark rugged outlines of the mountains by which the valley is bounded.

Between St Vincente and St Ann's some of the finest parts of the northern shore are seen. All along this coast the cliffs are of great height, and extremely rugged and precipitous. Here and there they are cleft down to the level of the sea by ravines in which mountain streams have found a channel. The sides and summits of the rocks are in most places covered with the dark foliage of the island trees. During the winter season, when in the high grounds the clouds and mists furnish a perpetual supply of water, numbers of small streams precipitate themselves from the top of this line of cliffs, forming a series of most beautiful cascades. Several of these sometimes occur within a short distance of each other, where the height of the fall is many hundred feet of nearly perpendicular descent. In some places where the summit of the cliff overhangs considerably, the stream is seen to sweep over, but becoming broken and scattered in its descent, is gradually altogether dispersed in mist; so that the dark volume of water seen at the top, appears farther down in the form of a white cloud borne by the wind along the face of the rock.

At some places, there is between the cliff and the beach a narrow tract of soil through which the road is carried. In passing these places, with such a scene of wild magnificence on the one hand, and on the other, the waves of the Atlantic, here ever troubled and boisterous, breaking in immense surges on the steep shore—the feeling inspired into the traveller is one of gloomy and desolate grandeur. During the greatest part of the year the wind blows with violence on the shore from the north; and there being no bays or anchoring ground for ships, and scarcely any shelter for boats, the coast is ever dangerous, and the sea altogether inhospitable for vessels.

The aspect of the island from the water, in approaching it from the north, is, however, extremely striking. The range of cliffs appears like a huge rocky wall, the base of which is lashed by the surges of the ocean, while its summit is crowned with a rich and luxuriant verdure. Beyond, appear successive heights, rapidly rising towards the central ridge of the island. A zone of clouds frequently rests about the middle of the mountains; while, far above, the apparent height greatly increased by this interception from the base, are seen a few of the peaks and lofty summits of the island. When these happen to be covered with snow, the effect of the view must be further increased; but the intercepting zone of cloud is not likely to be then present.

The road to St Ann's sometimes runs through the narrow tract between the rocks and the sea, which is partly cultivated ground; sometimes is carried across the high ground above; and in one place is at a great height cut in the face of the cliff for a considerable distance. It is much lengthened by the winding up and down the sides of the steep ravines by which, from time to time, it is interrupted. The site of the village of Ponta Delgada; the Arc and the ravine of St Jorge, are points of especial magnificence; but throughout, the road is of great interest. In approaching St Ann's, the path lies through a through [illegible] more open and cultivated, at a considerable elevation and outlin[illegible]ea; and the house where travellers find accommo- and at its out[illegible]ated in a district of extreme loveliness. The plantations,* and gardens, and vineyards form a paradise of rich and graceful scenery; and the beauty of the place, like that of the valley of St Vincente after crossing the mountains, is the more striking from the contrast with the wild and gloomy grandeur of the scenes through which the day's journey has lain.

Between St Ann's and Funchal are also some fine scenes, especially in approaching the neighbourhood of the principal river of the north coast, the Ribeiro Frio, the steep banks of which are covered with evergreen forests. At a little distance from the road, near the Ribeiro Frio, a path at the side of a Levada or water-course, leads through a pass in the mountains, at the end of which we look down upon the Meyo Metada, an abyss like the Corral, yet on a scale still more gigantic. This is certainly the finest point in all the scenery of Madeira. The depth of the valley must be very great, because the distance from the sea is not great, and the river has not a very rapid descent after escaping from the mountains; while the wall of the ravine opposite to where we stand, is formed by the precipitous sides of the Pico Ruivo and other highest summits in the island. It would be needless to attempt to describe the effect of the combination of rock and ravine, cascade and forest, which constitute this marvellous scene. Indeed, all of the mountain-district of Madeira presents to the traveller, at every step, scenes of which the pen of the poet or the pencil of the painter† could give but a feeble conception. The scenery at the same time is of a kind different from what, in almost any other country, can be met with, from the peculiar geological character of the formations of which it is composed, of which I will now give some description.

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* "In the North," remarks Mr Lowe (in Bot. Miscellany), "I have witnessed whole acres in the woods completely covered, in October, with the lovely flowers of the Amaryllis Belladonna; a scene exceeding in beauty even the dreams of poets."

† Many artists have endeavoured to delineate the scenery of Madeira, but of the views hitherto published, I have seen scarcely any that might not be set down as failures. Mr Bowditch, who has given some sketches in his work on Madeira, speaks, in reference to the Corral, of the "interest in a geological point of view of a delineation of the place;" and hopes that some "skilful hand will yet be able to give the world a correct view of that wonderful scene." By far the most successful attempt to effect this, has been made by Mr Picken, a young artist of great genius, who has resided for several winters in the island on account of his health. He has made some admirable paintings of the principal scenes, including the city of Funchal, the Corral, the Ravine of St Jorge on the north coast, and other sites alluded

*Geology.**—To the geological observer the island of Madeira everywhere presents the marks of igneous action. Whether

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to in this paper; and is at present preparing eight lithographic views for publication. To the geologist, these will give a very good idea of the appearance and physical geography of the island, and will be generally interesting as representations (as far as art can imitate nature where she has played at will her strangest fancies, wild above rule and art) of the most remarkable natural scenery in the world.

* For my information regarding the geology of the island I am chiefly indebted to Mr Smith of Jordanhill, who was in Madeira last winter, and in whose company I enjoyed many agreeable excursions in the island. Mr Smith wrote some letters on the geology of the island, which were translated into Portuguese, and published in a newspaper of the island, the Flor d'Oceano. In the same paper was reprinted, from the Transactions of the Lisbon Academy, a memoir by Senhor Mousinho d'Albuquerque, formerly Governor at Funchal. Sr. Mousinho's memoir is very valuable for the minute and accurate description given of the various pyrogenous rocks and minerals. Mr Bowditch gives some geological observations in his "Excursions in Madeira." In the first vol. of the Geological Transactions of London is a paper by the Hon. Mr Bennet, which refers only to one or two sections and appearances on the south coast. Many scattered remarks occur in the works of travellers, both English and foreign, who have touched at the island; but, for the most part, their observations are exceedingly limited, and often erroneous. Mr Smith first pointed out, from some fossils found at St Vincente, Mr Bowditch's error in describing the limestone there as transition; and determined the age of the island as an extinct volcano of the tertiary epoch. Mr Smith's remarks on this and other points were published in the Portuguese Journal already mentioned, which was conducted last winter by a young military engineer, Sr. Antonio Azevedo, a man of science and of varied acquirements, who labours for the promotion of science and the introduction of improvements in the island. The natives could not yet, perhaps, support a scientific association; but Sr. Azevedo and one or two of his friends published, in the Flor d'Oceano, papers and letters of an *ideal* academy, "The Philosophical and Mechanical Institute of Madeira," which, we trust, may soon be converted into a *real* and useful institution in this little "*Nova Atlantis.*" We endeavoured last winter to establish an English journal of literature and science, "The Stranger;" but, from the want of workmen and printing materials at the time, had to desist after publishing one small number. Every encouragement was given to the undertaking. We had the use of the Government printing-press; abundant materials were contributed, and the support obtained of many of the Portuguese

he examines the ravines in the centre of the island, or the cliffs on the coasts which the action of the sea has exposed to his view, the rocks of which the whole mass of the land is composed are found to be of volcanic origin. Some marine formations there are, but in a comparatively small proportion. Of the volcanic products, some have been thrown out in the form of fused lavas, others have been projected into the air, and others deposited in a state of aqueous suspension. Without giving any detailed description or enumeration of the various rocks, I may here state some of the more interesting points in the geological appearances of the island.

I. *Volcanic Formations.*

Basalts.—The igneous rocks are all of the basaltic class. They present great variety of structure, according to the temperature and pressure to which they have been exposed, their contact with atmospheric air or other gases, and other influences to which they have been subjected. The basalt is, for the most part, compact; but much of it is more or less vesicular, and some also scoriaceous. The compact basalts are sometimes, in structure, altogether homogeneous, but frequently contain a variety of minerals, especially crystals of olivine, which occurs in great abundance. In the vesicular basalt, the gaseous interstices, while compressed by the force of gravity, are also generally elongated in the direction of the current of the fluid rock. Observation of this is important in reference to the origin of these currents. The scoriaceous and cellular basalt occurs wherever the lava has been much exposed to the air, as in the smaller streams, and in the upper and outer surfaces of the great deposits. In such situations

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as well as of the English, of whom (including the strangers who reside in Funchal during the winter) there are about 500 in the island. It is much to be wished, that, when circumstances permit, this undertaking should be resumed, for (apart from the amusement that the literary department would afford in a place where there are not many sources of occupation to the strangers) there are few foreign stations from which, on many points in natural history, horticulture, and other subjects, a greater mass of valuable observations would be readily contributed, if a local medium of scientific communication were established.

the iron contained in the mineral also becomes peroxidated, so that the dark colour is changed to various hues of brown and red. In regard to the position of the basalt, the greater part exists in layers, or stratiform beds, of different degrees of thickness. These beds do not seem to follow any one general direction, but lie in an irregular manner throughout the island. They are also in various position with relation to the other formations, being interstratified with some, overcapping others, and appearing amongst others in the form of veins and dykes and amorphous masses. In some places the columnar form is assumed in great perfection. Of the basaltic dykes, some magnificent specimens may be observed on the road to St Vincente after passing the ravine of the Serra d'Agoa, where they are seen rising through the whole of the formations to the very summits of the mountains.

Basaltic Conglomerates.—Basaltic conglomerates of various kinds form the great mass of the mountains, and whole surface of the island. These conglomerates resemble each other in containing fragments of the same pyrogenous rocks, but present an immense variety, according to the nature, size, and quantity of the basaltic fragments which they contain, and the proportions of the volcanic mud and other products of igneous eruptions which enter into their formation. In some cases the compact dark basalt predominates, existing in globular masses, or in irregular fragments. In others the scoriaceous and cellular structure is more prevalent. Sometimes the fragments contained are so minute, that the conglomerate has a compact and homogeneous texture. With the various conglomerates the compact basalt everywhere alternates, and traverses them by veins and dykes. The position of these formations may be seen in the sections which are presented everywhere, both in the interior and along the coast. In the magnificent sea-cliff of Cape Giram, formerly referred to, various species of conglomerate, with basaltic dykes penetrating them, are finely displayed. The great mass of the high land of Madeira is of a structure analogous to what is there seen.

Along the sea-cliffs, towards Cape Grajam, various beds of rocks are seen, which, from the manner of their stratification, have evidently been deposited from aqueous suspension. They are composed of the same materials with some of the igneous rocks, but which, in a comminuted state, have been borne down during volcanic eruptions by the torrents of water produced by the condensation of the vast volumes of vapour then evolved. The Cape is named by the English "Brazen Head," from the red colour of the strata, which in that situation being much disturbed, and intersected by dark basaltic dykes, present from the sea a very peculiar appearance. Immediately to the west of the Pontinha, a similar aqueous deposit may be observed along the cliff. In this locality it is overcapped by a thick bed of compact basalt, which has been poured out over it. The heat of this superincumbent mass has given rise to an interesting appearance. The upper part of the layer of the red aqueous stratum, which has been in contact with the basalt, has been altered by the intense heat, and, in cooling, has assumed the columnar structure. A very fine effect is thereby produced, the dark amorphous mass of basalt being supported by lines of small red pillars of an angular form. This is seen for a considerable distance along the coast, but the appearance is especially beautiful at the mouth of the small stream the Ribeiro Seco, where a large extent of this columnar formation is exposed.*

Aerial Volcanic Rocks.—Between the points just mentioned, the Pontinha and the Ribeiro Seco, may be also observed a series of aerial volcanic products. They consist of pumice, sand, and the various matters ejected by the expansive force of vapours or gases. The different layers, some of which are several feet in thickness, are well marked, denoting successive volcanic showers of different materials. Some of the layers consist of white lapilli of pumice, which have very small specific gravity, and float on the surface of water. These layers of light pumice alternate with various beds of heavy dark tuffs and sand. This disposition of the layers proves that they have been deposited on the land and not at the bottom of the sea, where they would have taken their place in relation to their specific gravity. In several places the beds dip down under the present level of the ocean, proving, that, since their deposition, there have been depressions in the surface of the land in these parts.

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* Mr Bowditch (Excursions, p. 24) remarks, that the red tufa, which has undergone this change of form, is increased in specific gravity from 1.75 to 2.06. I am not aware whether this effect of volcanic streams, in thus determining a columnar structure in previously deposited rocks, is of frequent occurrence, or whether it has been described in reference to any other locality. Mr Smith informs me, that near Glasgow a whin dyke passing through a coal bed has deprived the coal of its bitumen, and rendered it columnar. Since being in Madeira, I have observed (June 1840) a fine appearance of the same kind on the south side of the hill of Gergovia, near Clermont in Auvergne, where the basalt has flowed over limestone, and caused it to assume, in like manner, the columnar form, the little pillars for some distance presenting considerable regularity.

II. *Formations not Volcanic.*

Along the same sea-cliffs, in the vicinity of Funchal, are seen beds or strata of matter, not of volcanic origin, lying beneath some of the basalt and other pyrogenous products. In these beds are found the roots of plants encrusted with mineral matter. From the number of such vegetable remains, it is probable that the chief materials of this earthy deposit consist of the old vegetable soil which covered the island. At the eastern extremity of the island there exists a remarkable formation, which has been thought analogous to these earthy beds, but to contain vegetable remains in far greater abundance. It is termed generally the Fossil Forest of Caniçal, which is the name of the parish in which it occurs. Believing the general opinion regarding its nature to be erroneous, I will refer at greater length to this formation.

Fossil Formation of Caniçal.—The Point St Lourenço, which forms the eastern extremity of the island, extends about three miles into the sea with an inconsiderable breadth. The land, immediately before where it becomes contracted to form the Point, and nearly to the Cape at the extremity, consists of beds of basalt and of conglomerates, as I have described most of the high grounds to be composed. In sailing along the south coast from the town of Machico, from which visitors generally set out to visit the fossil bed, the structure of the formations on the lofty coast are finely seen; different layers of conglomerates, globular and scoriaceous, traversed every where by veins of compact basalt; and in some places there appear masses of the latter rock in the columnar form. The rocks are of less height as we advance eastward; but the line of the coast is at intervals marked by masses of basalt, and in one place a hill of some height remains, the last high ground on the south coast, on the summit of which is a chapel of the Virgin, from which the rock takes its name, Monte de Nossa Senhora da Piedade. On the north side of the island, the cliffs retain a great elevation, for some distance beyond this point.

Immediately to the west of this chapel mount we land in a small bay, where is the only piece of sand beach I believe in the island. The distance to the opposite coast may here be about a mile or more, the ground sloping gradually from the north coast, where the cliffs are from two to three hundred feet in elevation. Over the whole of this extent, and for a considerable space to the eastward and westward, the pyrogenous rocks are covered with a deep bed of calcareous sand, containing vast quantities of what has been described as remains of former vegetation. The formation is thus described by Mr Bowditch:—"The sands have been in some degree fixed or bound by the numerous branches of forest trees which they have enveloped; for these branches (which have preserved their lateral twigs) are so numerous, that they are spread over the surface, like a network of stoloniferous roots. It is scarcely possible to set the foot on the ground without treading on them. Both the branches and the trunks (which stand on their roots in their natural position) are encased in a thick hard sheath of agglutinated sand, which has followed the external configuration of the wood like a cast. In some instances, the wood has entirely perished, and the envelopes are found void like tubes, but most frequently the wood is found within, as a distinct mass, and has become sufficiently siliceous to scratch arragonite. Sometimes imbedded in the envelopes of the wood, but generally in the loose sand of the surface, were innumerable fossil shells, intermingled promiscuously, two species terrestrial, the third belonging to a marine genus. The Delphinula approaches the *D. sulcata* of Lamarck, only known in the fossil state, and found at Grignon. Both helices belong to the group *Lamellatæ* of De Ferrussac's subgenus *Helicostyla*. These shells are perfectly distinct from the existing helices of Madeira. All the branches and wood appear to belong to the same sort of tree (of which there seems to have been a small forest on that spot), and that evidently a dicotyledon; but more than that I do not think our present knowledge of the comparative anatomy of timbers is sufficiently advanced to determine."*

Albuquerque also describes the locality in great detail, and has the same view of its nature; mentioning that the branches and roots, even the minutest ramifications, are so obvious, as to leave no doubt of its being a vegetable formation. He considered it as coeval with the beds of vegetable soil which I have alluded to as occurring along the coast in the vicinity of Funchal.

The formation has quite the appearance of what it is represented by Bowditch to be. The masses contained in the sand in form and structure seem to be the stems and roots, not of forest trees, indeed, but of a tract of heath or underwood. Of this I was so much satisfied, that in my notes made at the place, I find I have marked it as probable that the plant was the *Vaccinium Maderense*, which, throughout the island at present, forms large thickets, similar in appearance to what we can conceive this to have been. I visited the formation in company with Mr Smith, Mr Buchanan Hamilton of Leny, and Dr John Russel of Edinburgh, and none of the party had any doubt as to the vegetable origin of the remains.

Concerning the theory of the formation various opinions have been submitted. Bowditch says "it must evidently have been from an irruption of the sea, from the heaps of terrestrial shells mingled with the marine; and from the trees being found standing on their roots, and not deposited promiscuously, or flattened as by pressure of a superincumbent stratum afterwards removed." The calcareous sand he thought to be derived either from destruction of fragments of limestone in the bed of the ocean, or from comminuted shells; more probably the former. Mr Smith, with much more probability, thought that it was an ancient wood, sanded up by a blown sand, composed of minute fragments of basalt and comminuted shells, the same as forms the beach in the little bay or landing place. The structure of the plant thus sanded up he thought to be replaced by carbonate of lime from the percolation of water; the lime possibly supplied by the decomposition of the shelly fragments.

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* Bowditch's Excursions, pp. 139, 140.

To these, or any of the explanations that have been offered, various objections might be made; but the chief difficulty left unremoved by them, seemed to me the accounting for the vast quantity of calcareous matter in the formation; far more than the lime borne along with an irruption of the sea, or resulting from the decomposition of the scattered specimens of shells could possibly supply. For not merely the fossils, but the great mass of the sand-formation in which they lie is calcareous.

Calcareous deposits, similar to that of Madeira, have been described by voyagers as occurring at the Cape of Good Hope, and in New Holland. Of the latter, the most recent account is given by Mr Darwin in the valuable and interesting volume recently published, containing the observations in Geology and Natural History, made in the expedition of H. M. Ship Beagle. In speaking of Van Diemen's land, he says:* "One day (March 1836) I accompanied Captain Fitzroy to Bald Head, the place mentioned by so many navigators, where some have imagined they saw coral, and others petrified trees, standing in the position in which they grew. According to our view, the rock was formed by the wind heaping up calcareous sand; during which process, branches and roots of trees, and land shells, are enclosed; the mass being afterwards consolidated by the percolation of rain-water. When the wood had decayed, lime was washed into the cylindrical cavities, and became hard. The weather is now wearing away the softer rock, and in consequence the casts of roots and branches project above the surface. Their resemblance to the stumps of a dead shrubbery was so exact, that, before touching them, we were sometimes at a loss to know which were composed of wood, and which of calcareous matter."

I have examined the accounts given by former travellers of this place. It was Vancouver who first observed it in 1790.

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* Journal of Researches, &c., by Charles Darwin, Esq. Sec. Geological Society. London, 1839, p. 537.

He describes it[*] as "a formation of sand, extending over about eight acres, through which protrude branches of coral, with ramifications of different sizes, some not half an inch, others four or five inches in circumference." The top pieces are soft, and easily reduced to powder; the lower are more compact in texture. Sea shells abound over the whole surface. Captain Flinders also visited the locality.[†] A French navigator, Peron, accounts for the formation in the same way that some have supposed that of Caniçal to have been formed. He supposes[‡] that "shells cast on shore are decomposed, and having lost a portion of their carbonic acid, approach to the state of the lime used in some calcareous cements; and in this state unite into a compost with quartz, and form incrustations on the surfaces of plants. As the wood is destroyed, the mass becomes gradually a mere sandstone, and nothing but an arborescent form indicates the ancient state of vegetation."

Of the formation in South Africa, Mr Clarke Abel gives an account in his Chinese Journal. "Somewhat to the eastward of Simon's Town is a large bank, which rises from the sea to the height probably of a hundred feet, and seems to have been formed by an accumulation of shells and sand brought up by the south-east wind. On this bank a great number of cylindrical bodies lie scattered about, and at first sight resemble the bones of animals bleached and disorganized by exposure to the air. On a closer examination, many of them are found to be branched, and others are discovered rising through the soil, and ramifying from a stem beneath thicker than themselves. Their vegetable origin immediately suggests itself, and is confirmed by further inquiry."

Mr Abel says,[§] that he "compared a specimen from New Holland with those from the Cape, and could trace no essential difference in their external character, and that, when subjected to a similar chemical analysis, they gave precisely similar results. As this specimen," he adds, "has a remarkable resemblance to coral, both in form and closeness of texture, it may perhaps be considered a fair example of those substances considered coral by Vaucouver and Flinders. If this be admitted, it will follow that the reasoning is incorrect which is founded on their supposed submarine origin."

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[*] Vancouver's Voyage, vol. i. p. 48. Ed. 4th. 1798.

[†] Flinders' Voyage, vol. i. p. 63.

[‡] Quoted in Abel's Narrative of a Journey to China, p. 308.

[§] Abel's Narrative, p. 311.

After returning home from Madeira, I had some sections of the Caniçal structure made, thinking that they would form good objects of microscopic inspection, as specimens of fossil wood. In these sections there appeared, however, not the slightest form or aspect of vegetable structure. The arrangement of the calcareous matter was altogether similar to what is found in corallines and marine animal productions. Their vegetable structure was therefore rendered somewhat doubtful. On heating a fragment in a glass tube, a great quantity of ammonia was given off; which confirmed the idea of the animal structure of the specimen, and shewed that the organic matter had not, as was supposed, been wholly replaced by the carbonate of lime and mineral deposits.* The formation is, therefore, a tract of fossil coral, belonging probably, from the appearance of the structure, to the family of Alcyonidæ: the

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* The following is a detailed analysis which Mr Thomas Anderson of Leith has had the kindness to make for me.

When heated to redness in a tube open at both ends, the fossil gave out a strong smell of burning animal matter, becoming after a little powerfully ammoniacal.

Dissolved in diluted nitric acid, a brown powder remained, which, when ignited, left silica, the organic matter being dissipated.

To the solution, rendered nearly neutral by evaporation, an excess of ammonia was added, which threw down phosphate of ammonia.

Carbonate of ammonia precipitated carbonate of lime.

Quantitative Analysis:—

Carbonate of lime, . .	73.15
Silica, ...	11.90
Phosphate of lime, . .	8.81
Animal matter, . . .	4.25
Sulphate of lime, . . .	a trace.
	98.11

This analysis, along with the sections, I think satisfactorily establishes the animal origin of the formation.

species being perhaps allied to, if not, the *Alcyonium arboreum* of the present time.

The Madeira formation is, therefore, either different from those with which it has been classed; or the observations of Abel, Darwin, and others, who have sought to establish their vegetable origin, have been imperfect. Of this I cannot determine, not having seen specimens from the other localities. The Madeira formation, however, is an interesting one, although it be different from the others, on account of the extent of the tract which it covers. With regard to the shells that abound on the surface of the formation, I have already quoted Mr Bowditch's observations. Mr Lowe has added many to them. I have not a catalogue of those determined by Mr Lowe, but remember his stating that about one-fifth of them are extinct species. They belong, therefore, to the upper tertiary or Pleistocene epoch of Lyell. This, too, argues against the vegetable nature of the formation; for it is not likely that a wood of *dicotyledonous* trees or shrubs (to the appearance of which alone the specimens bear resemblance) would have existed at a time anterior to the extinct shells that have been deposited above them.

This, then, seems to have been the origin of the formation. A large tract of coral, of the Alcyonian tribe, has been upheaved, by the elevation of the pyrogenous rocks from submarine volcanic action. The death and decay of the Alcyonia ensued, leaving the soil covered for a great extent with a forest of coral, the surface of which, from exposure to the sun and elements, has been in great measure worn, and still is rapidly crumbling down, forming a calcareous sand similar in composition to that of the specimen. Mr Darwin describes an analogous formation (" considerably more than a mile square, covered with a forest of branching coral," p. 547.) produced in Keeling's Island, and by a similar cause, namely, the death of the coral over a large extent, from exposure to the sun, arising from change of level in the surface. There, however, the animal is that of the common coral, and the change of surface was produced by a gradual change of the surface of the ocean at the place. In Madeira the coral is an Alcyonium, and the exposure was produced by a violent change of level from volcanic agency. The extinct species of shells, and other appearances (such as a fragment of an extinct monocotyledonous vegetable, apparently a Lycopodium, found by Mr Smith) presented on the surface of the formation, shew that its age is before the latest part of the tertiary epoch. If this account of the matter be correct, I think the difficulty is satisfactorily removed as to the enormous amount of calcareous matter (the origin of which Mr Bowditch and others have had to account for by such inadequate causes as the comminuted sand of distant limestone beds, or the decomposition of the shells scattered on the surface of the ground), for the Alcyonia would give from their own structure all the carbonate of lime which appears; and, indeed, in many places, the coral may be seen in every stage of decomposition, the specimens everywhere crumbling down into the amorphous soil of calcareous matter of which great part of the surface of the formation is already composed.

Thus far an induction from observed facts seems to lead. Whether the coral be an extinct species, or the *Alcyonium arboreum:* whether the formation be similar to those with which it has been classed, or of a nature not elsewhere as yet observed: how there come to be found on the surface such a vast number of land molluscæ:* and various other questions,—remain for future investigation.

Between St Jorge and St Ann's there appears a bed of lignite, on the bank of the river of St Jorge, immediately before the entrance of a stream called the Ribeiro de Tabaco. Sr. Mousinho describes it as lying above a bed of hard clay impregnated with lignitic matter, which rests immediately on basalt; the layer of lignite itself being covered by a thick basaltic formation.

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* It has been observed that calcareous rocks have more influence than any others in increasing the number and propagation of molluscæ. (See Edward Forbes' Report, p. 128, in Transactions of British Association for 1839.) The presence of this calcareous tract in a country almost wholly basaltic, may account for the remarkable accumulation of shells in this spot, while they occur scattered rarely in the rest of the island.

Mr Bowditch was wrong in stating that marine shells occur here. Mr Lowe states (Primitiæ Faunæ et Floræ Maderensis, Camb. Trans.. vol. iv, p. 64), "Nullam quidem speciem marinam cum illis (terrestribus) commixtam vidi. Delphinula sulcata (Bowd. Excursions, p. 140) est Helicis species (H. delphinula Nob)." All the conical shells are terrestrial species.

The last and most important of the non-volcanic formations is the limestone occurring near St Vincente. About two miles up the valley, in a narrow branch ravine on the eastern side of the river, and at an elevation of more than 1000 feet above the sea, there is exposed by the mountain torrent a mass of limestone surrounded on every side by the basaltic rocks. The remains of one or two small kilns are near the spot. The small extent of the formation that can be exposed, and the great difficulty of transportation, must have led to the abandonment of the working of the lime for building purposes. Mr Bowditch's account of "a bed of *transition* limestone *seven hundred feet thick*," and referred to by Dr Daubeny (in his book on Volcanoes, p. 261.) and others, is altogether erroneous. The account he gives is as follows: "This limestone is crystalline in its texture, contains very little siliceous matter, and scarcely any compact masses; yet, from the great depth of the bed (being nearly 700 feet from its junction with the superincumbent basalt, to my last glimpse of it in the bed of the torrent, nearly level with the sea), without a single alternation, I have no doubt of its being transitive rather than primitive limestone." P. 51. With regard to the extent of this formation, I believe that only a small portion of rock is exposed, and that quite isolated by the igneous rocks which have raised it to so remarkable an elevation. Mr Bowditch may have observed other portions *in situ* 700 feet below the upper station; but assuredly there is no bed of that thickness "without alternation." In ascending the bed of the mountain stream, I observed large masses that had been borne down from above, which may have been the source of the mistake. If the limestone existed "in the bed of the torrent nearly level with the sea," the lime-kilns would not have been built at the upper limit of the formation, where the difficulty of transportation has partly caused them to be abandoned. Again, with regard to the structure of the rock, it contains many fossils of the tertiary series. Mr Smith obtained specimens of the following genera, Venus, Tellina, Crassina, Astrea, Pecten, Cardium, Fasciolaria, Murex, Voluta, Cypræa, most of the apparently extinct shells, with some corallines and zoophytic remains. The conclusions of Mr Bowditch (at page 68, "Excursions," &c.), with others founded on them, based on the idea of "a vast bed of transition limestone of a depth of 700 feet," therefore, fall to the ground.*

From the preceding notes, although I have not entered into any details, it will be readily perceived that the island presents a fine field for geological study. Even from the few facts that have been referred to, many interesting conclusions might be drawn.

It appears probable, from the similarity of their volcanic rocks, that there exists between the Madeira and the Azores, the Canaries, and other islands in that part of the Atlantic, an intimate geological connection. In all, there abound the same basalts and conglomerates, and layers of scoriæ. In Teneriffe and St Michael, and other islands where the volcanic action is still in activity, these formations are covered by many series of more recent deposits, the ancient and modern lavas being distinguished from one another. The volcanic action having been long extinct in Madeira, there are found none of these modern deposits, but its rocks are the same with the older series in the other islands. This similarity has led some geologists to revive the speculation concerning a former continent or large island existing in these parts; the Atlantis perhaps of the ancients. In addition to what has been urged in this matter by Bory St Vincent and previous writers, Sr. Monsinho, in the memoir already quoted, supports the idea, and considers Madeira as a portion of a vast region which has been broken up and in a great part swallowed by the ocean. If this fancy were not by many other proofs shewn to be groundless, a very slight consideration of the geological appearances of the island would prevent its being seriously entertained. The nature and the amount of the non-volcanic formations, the position they occupy amidst the pyrogenous products, and the manner in which the various rocks are situated, sufficiently demonstrate that no former track of land has been submerged, but that the island has, by the force of submarine volcanic action, been raised to its present state; and that the older strata, fragments of which now appear even at a great elevation, were originally formed at the bottom of the sea, and remained there till disturbed and heaved up by the igneous agency. That the present Archipelago of Madeira is but the fragment of a greater region, Sr. M. d'Albuquerque further argues, from the absence of any focus of divergent currents, and the general inclination of all the beds of lava, which he says is in the direction of south-west. This inclination is certainly far from obvious; for, as already stated, the beds lie in various directions, and seem to diverge not from one but from many foci of volcanic energy. There is certainly no where to be seen anything like a well-marked crater, such as those of Auvergne and other districts of extinct volcanic action. Some have considered the Corral to be the crater of the island. I believe it to have been only one of the several centres of eruption indicated by the directions of the basaltic beds. Another, for instance, and equally well-marked, seems to have had its site in the present bay of Funchal. The outline of this crater may be traced partly by the line of basaltic rock at the Pontinha, which, running out into the sea, forms the pier there, and part of the continuation of which is seen in the basaltic island on which the Ilheo fort in the bay is built. Other centres of eruption are obvious in different parts of the island. The only difficulty in the way of supposing these to have been the volcanic craters of the island is their immense size. But every thing around bears token of the gigantic scale on which the igneous agency must here have been in operation. The vast volume of the basaltic formations, the thickness of the strata of conglomerate and volcanic sand, and the enormous quantities of scoriæ and other ejected matters, prevent any surprise as to this matter. Even in the comparatively tranquil state of things since the commencement of the present geological epoch, these seas have been the site of subterranean and submarine volcanic action on the vastest scale. Since the year 1445, when the Portuguese navigators found a new island of great size raised above the sea, till our own times, as in the instance of the island of Sabrina, which was more than three hundred feet above the sea and a quarter of a league in circumference, many similar occurrences have been recorded, proving the extent and power of the igneous agency, yet* operating in this district of the eastern Atlantic.

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* In the adjoining island of Porto Santo, there is a recent tertiary limestone, also abounding in fossil shells. I did not visit this; but at the lime-kiln near Funchal, which is supplied from it, I collected many specimens. I have not yet had my specimens determined. One or two appear the same as species found at St Vincent's. Many of them are probably different species from any that have been elsewhere discovered.

In regard to the geological age and history of the island, the fossils are not yet sufficiently known to admit of great precision of statement. From the fossils of the limestone of St Vincente, the oldest formation that has been observed, it appears that the island was first raised from the sea during the tertiary epoch.

The volcanic fires must have remained in activity, with intervals at least of repose, during a long series of ages, because the different formations indicate many eruptions distinct in their character. Much of the island seems to belong to the very latest or Pleistocene ages of the tertiary epoch.

It seems further probable, that the conglomerates, of which the great mass of the island is composed, as well as much of the compact basalt, were formed beneath the ocean, and raised by the force of elastic vapours and the upheaving of successive volcanic products; the island being chiefly the product of submarine volcanic action. The beds of scoriæ and light lapilli, and the series of basaltic formations subsequent to them, shew that the island was for a long period also a subaerial volcano. It is farther apparent from those pumice beds (originally deposited horizontally above the sea level, but afterwards disturbed, and now dipping down at considerable angles under the ocean) that, after the time of their deposition, the relative level of sea and land was in many parts by violent means altered, and the island partially exposed to the influence of those causes, by the operation of which, many volcanic tracts that have been raised above the sea have been again made wholly to disappear. This violent agency on the surface of the land is further manifest from the vast size of the valleys and ravines, which the small streams that now run in them could never in any course of time have been able to excavate; as well as from the abrupt and rugged character of the gigantic cliffs which now form in most parts the outline of the island. These could only be the effects of earthquakes or other great convulsive agencies. Since the commencement of the present geological epoch, the island has been in repose, there being no indication of recent eruptions, and none of the products observed in modern volcanic districts.

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* Last winter, on the 5th of December, the whole of the south coast of St Miguel, one of the Azores, was inundated by the sea, which suddenly rose upon the land with prodigious force, destroying many houses, bridges, and other buildings, and causing great havoc and loss of property. We heard of the disaster soon after in Madeira, but no trace of the submarine disturbance was observed there.

The changes at present operating on the surface of the land are only those of a gradual nature. For many years no shock of an earthquake or other violent disturbing cause has been experienced. When these have occurred, they have been connected with distant volcanic action. In many places, the waters of the ocean are rapidly wearing the sea-cliffs. In the interior, the rivers work a considerable change at times upon the surface. From the steepness of the sides of the ravines, there are frequent slips of the soil, the havoc of which adds to the romantic wildness of the scenery of these places. Although none of the rivers are ordinarily of great size or power, they are sometimes swollen so much as to bear down immense quantities of soil and rock, and work great damage in their course. In the year 1803, for instance, all the streams were swollen by a great flood, by which an immense amount of damage to property was sustained, and in some places bridges were destroyed and many houses borne down by the force of the waters.

The sea around the island is almost every where immediately deep, as might be expected from the abrupt declivity of the shores. The configuration of the adjacent submarine tract, so far as known, is extremely irregular, there being no plains of great extent, and those which do exist being composed of rough and rocky surface, without any sand or comminuted detritus. In the Bay of Funchal, which is the principal, and indeed the only station for large ships, the depth of anchorage is from forty to fifty fathoms, and the bottom broken and rocky. In those places where the sea does not wash the face of the cliffs, there is a bank of coarse shingle, formed of rounded masses of basaltic rocks. With one or two trifling exceptions, there is no spot with a gravel far less a sand beach. The absence of a beach in a maritime station is one of the few wants felt by stranger invalids in Funchal. The want is in a great measure supplied by the terraces that have been made along the shore, which, when the trees have grown sufficiently to afford shelter from the sun, will prove most delightful places of resort for walking. The sea bank is generally very steep, and over it there is every where more or less of a surf. In the bay of Funchal, which is well sheltered from most winds, this surf does not give rise to much annoyance; but even there it is often difficult to land a boat with safety. It is most amusing to observe the skill with which the islanders convey to shore boats heavily laden with fruit or other cargos which the ingress of the sea water would destroy. The surf breaking close into shore, the boat is kept in the smooth water immediately beyond it, within a few feet of the beach, till the boatman on the look out, who has patiently allowed many a wave to break, descries one approaching which he deems favourable, and the signal having been given, the boat is borne on it securely to land, and immediately drawn up out of reach of the succeeding wave. The process seems a very easy one; but I have more than once seen English sailors, who thought so and tried it, fail, and get thoroughly ducked, to the great amusement of the native boatmen.

The prevailing winds of the island are northerly and easterly. Perhaps two hundred days in the year may be noted as between the N.E. and N.W. points; and about fifty more due east. From this it is obvious that the island is within those limits in which the trade winds blow with considerable regularity. Occasionally there are violent irregular winds which do considerable damage to the vines and other property. These hurricanes, are, however, of rare occurrence. There is one kind of east wind denominated L'Este, similar to the Harmattan of the African continent. It is a warm, dry wind; and, from the rapidity with which it increases evaporation, it is apt to cause great oppression to some constitutions, and in all to give rise to a parched and uncomfortable sensation of the surface of the body. Sometimes it blows for a considerable period, and with violence; and on these occasions, birds and showers of locusts are often borne with it from the continent of Africa. Twice during last winter, I procured specimens in this way. During the continuance of the L'Este, the hills present a peculiarly clear and cloudless aspect, from its rapidly dissolving all moisture in the atmosphere. During winter and spring, the upper parts of the island are generally colder than the atmosphere that is continually moving southward over it, much of the vapour of which is, therefore, precipated, so that clouds and mist frequently envelope the tops of the mountains. In summer, however, the island always possesses temperature sufficient to suspend all the aqueous vapour borne over it by the winds of the sea; and weeks and months of sunny sky and cloudless weather, therefore, follow in uninterrupted succession.

The nights in Madeira are of surpassing beauty. The moon displays a radiance, to the brilliancy of which any approach is seldom made in this country. Venus, too, shines with beautiful refulgence, casting a shadow from objects. The lunar rainbow, a meteor never or rarely seen in our country, is said to be there of frequent occurrence, which indicates a remarkable clearness of the atmosphere. Twice during last winter, I observed the appearance. On one of the nights, in the month of March, it was visible, on mist or clouds, on the mountains for two or three hours, in distinct and beautiful display, while the full moon was not far above the horizon. The brilliancy of the heavens, the serenity of the air, the genial mildness of the atmosphere, render the nights, especially "when the moon with more pleasing light, shadowy sets off the face of things," more inviting even than the day to be abroad in. The absence of chillness and damp here, permits one with safety to enjoy this, "the pleasant time, the cool," but not "the silent;" for many of the natives, indolent during the day, then delight in their gardens and terraces; and the air is filled with the music of the guitar, and a sweet little instrument, peculiar to the island, the machettinho. The air is then, too, redolent with the sweet aroma of the orange and citron groves; and heliotropes, daturæ, jessamines, roses, with many a "flowery odour" besides, unite their tribute to increase the delicious fragrance of the atmosphere.

The climate of Funchal and its immediate neighbourhood, which is the only part of the island in which invalids reside during winter, has been ascertained by many series of most accurate observations. The advantages it presents over the best climates on the continent of Europe, have been set forth in a very clear light by Sir James Clark in his work on Climate. "The mean annual temperature of Funchal is 64°, being only about 5° warmer than the Italian and Provençal provinces. This very moderate mean temperature, relatively to its low latitude, arises however from the summer at Madeira being proportionally cool; for, while the winter is 20° warmer than at London, the summer is only 7° warmer; and, whilst the winter is 12° warmer than in Italy and Provence, the summer is nearly 5° cooler. The mean annual range of temperature is only 14°, being less than half the range of Rome, Pisa, Naples, and Nice. The heat is also distributed through the year with surprising equality, so that the mean difference of the temperature of successive months is only 2°.41; this at Rome is 4°.39, at Nice 4°.74, at Pisa 5°.75, and at Naples 5°.08. Whilst there is much equality in the distribution of temperature through the year, there is no less so in the progression of temperature for the day; the mean range for the twenty-four hours being 10° by the register thermometer, while at Rome it is 10°, at Naples 13°, at Nice 9°, by the common thermometer, which gives only the extremes observed during the day. The steadiness of temperature from day to day also exceeds that of all the other climates. In this respect, it is not half so variable as Rome, Nice, or Pisa, and is only about one-third as variable as Naples. The degree of variableness from day to day at Madeira is 1°.11, at Rome it is 2°.80, at Nice 2°.33, and at London 4°.01. The annual range of atmospheric pressure is also very small, being about the same as that of Rome and Naples. Nearly the same quantity of rain falls annually at Madeira as at Rome and Florence, but at Madeira there are only 73 days on which any rain falls, while at Naples there are 97, at Rome 117, and at London 178. The rain at Madeira falls at particular seasons, chiefly in the autumn, leaving the atmosphere, in general, dry and clear during the remainder of the year. From this comparative view of the climate, it must be readily perceived, how great are the advantages which this island presents over the best climates on the continent of Europe. It is warmer during the winter, and cooler during the summer; it has less difference between the temperature of day and night, between one season and another, and between successive days; it is almost exempt from keen, cold winds, and enjoys a general steadiness of weather, to which the best of these are strangers; the rains are circumscribed, and generally fall at regular and stated periods."

I have examined several meteorological registers kept with great accuracy and regularity, and have not observed the temperature in Funchal to have been, above three or four times in many years, below 50° Fahr. The lowest that we had last winter was 49°, on the morning of the 29th March, when there was snow on the mountains to within about 2800 feet above the sea. The station was, however, at a little distance from the city. The greatest monthly range during last winter was also in March, the maximum being on the 25th of March 70°. During the summer, the thermometer is as rarely above 80°, except during a continued l'este, when it has been observed several degrees higher in the shade.

I have not seen any good series of hygrometrical observations regarding the climate of Funchal. Judging empirically, from such effects as the rapid rusting of certain instruments, and the great difficulty of drying and preserving my botanical specimens, as compared with experience in other places, I should say, that, in general, the atmosphere is charged with an unusual amount of moisture. It never, indeed, appears in mist or fogs, or any other form of sensible humidity, because the temperature of the air by which it is suspended is so admirably regulated. The large amount of moisture, combined thus with a temperature capable of always retaining it in a condition in which it is not injurious to those states of disease, for which invalids ought to be sent to Madeira, I consider one of the chief elements in the excellence of the climate of Funchal. So effectually is the temperature adequate to prevent the appearance of any sensible humidity, maintained by the perpetual motion and mingling of the regular breezes and atmospheric currents, that, notwithstanding the great quantity of moisture in the air, and the facility of radiation in the extraordinary clearness of the nights, dew is scarcely ever deposited.*

The nature of the vegetation around Funchal displays the genial warmth of its climate. The surrounding country has, in many respects, quite a tropical appearance. The hill sides are covered indeed with vineyards, amidst which are scattered orange trees and cypresses and fig-trees, with hedges of rose and myrtle; and the inhabitants of the flower-beds, too, are much the same as in the south of Europe. But, at the same time, over the cottages and cabins of the peasants, the banana waves its broad leaves; the gardens are filled with the custard apple, the guava, and other fruits of the West Indies; there are large plantations of coffee; the cotton-tree and the sugar-cane flourish; the rocks, to a great height above the sea, are covered with a southern cactus (the opuntia tuna); and every where tropical flowers, in the greatest luxuriance, abound.†

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* These remarks, and most of what is said in different works concerning the "climate of Madeira," refer, it is to be noted, only to the *immediate* vicinity of Funchal. They may not be applicable at a very short distance above the city, or in other places on the coast.

† In Loudon's Gardeners' Magazine for October 1838, vol. xiv., p. 449, there is a description by Dr Lippold (a German botanist and natural history collector in Madeira and the Canaries), of the villa and garden of Dr Renton, at the Val. This garden is nearly 500 feet above the sea, and is in a most beautiful situation. The ground is laid out in terraces; and the culture, irrigation, and all the arrangements of the place, indicate great skill and taste in the management of a garden which has the richest natural advantages. The luxuriance and the diversity of the vegetation is astonishing. Dr Lippold's list contains a great variety of rare fruits and flowers, and shews how remarkably the trees of Australia, and of most other warm countries, thrive in the climate of Madeira. The garden of the Mount Villa (belonging to Webster Gordon, Esq.) is also extremely rich in valuable plants. The grounds are laid out with great taste, and present more variety of surface and expo-

The vine is not much cultivated beyond 2000 feet above the sea. From this height to the summit of the first range of mountains (which form the boundary of the view from Funchal) there are forests of chestnut, pine, and other European trees, with thickets of broom, heath, and furze; and the violet, foxglove, thyme, vinca, are among the flowers that characterize the zone of vegetation. The Serra de St Antonio is in altitude about the upper limit of the region; and on it, as I have said, the tree heath and Madeiran bilberry grow luxuriantly. At the same elevation begin to prevail the laurels and other native evergreens. Of these the most conspicuous are the *Laurus Indica*, or Madeira Mahogany tree; the Til (*Laurus fœtens*), the *Laurus Canariensis;* the *Myrica Faya;* and some of the *Taxeæ*. Between the upper limit of the vines and that of the laurels a great variety of ferns, and also many indigenous Compositæ and Labiatæ, are found. On the highest summits, the verdure seems chiefly to consist of grasses or of heaths.

From these few remarks the variety of climate, as indicated by the vegetation, is sufficiently obvious. The range of elevation, however, is too limited; and the vegetation presents too few marked transitions, to render the description of any fixed zones of distribution a subject of much interest or importance. That which strikes every one is the astonishing combination of climates, and this depending more on particular aspects and situations than on mere elevation. On the north side of the island there is little variety of vegetation perceptible, excepting in some of the valleys and sheltered recesses; and it is only in a few spots of the south that tropical plants can flourish, and the range of vegetation be thereby increased. The extent of this range is remarkably illustrated by the variety of fruits that come to perfection. Dates, guavas, limes, citrons, bananas, and a host of other tropical fruits, are in the gardens below; while above, the apple, nectarine, gooseberry, chestnut,

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sure than most of the Madeira quintas possess. The height above the sea being about 2000 feet, the cultivation of many of the plants of colder climates is permitted; and part of the ground has quite the appearance of an English garden. There are also some plantations of European forest-trees. The orange-tree flourishes, but the cultivation of the vine does not reach so high.

strawberry, and our European fruits and esculent plants ot every sort flourish.

> "Whatever Earth, all bearing mother, yields
> In India East or West, or middle shore
> In Pontus or the Punic coast, or where
> Alcinous reigned; fruit of all kinds, in coat
> Rough or smooth rind, or bearded husk, or shell,—"
> *Paradise Lost*, v. 338.

—the produce of every clime is heaped together; and the flora* presents a like union of endlessly varied vegetation. In this respect, indeed, the happy confusion of climate described by the poet is exemplified:

> Hic ver assiduum atque *alienis mensibus æstas.*—*Georg.* ii. 149.

To an English botanist it is delightful to go forth at a season of the year when at home scarcely a moss will reward his laborious search, and speedily fill his box with plants in full flower, and most of them entirely new to him. The change is remarkable, from being within a distance of only eight or ten days from home. On one morning in December, on the sea-cliffs, a little east of the city, I gathered, along with many other good plants, the following in flower:—*Matthiola Maderensis*, *Lavan-*

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* The indigenous flora of the island is now with difficulty distinguished, on account of the multitude of plants that have been introduced. It does not seem so rich as the variety of climate might lead one to expect. Probably it does not much exceed 300 species of phenogamous plants; but many of them are interesting to the botanist, as being peculiar to the island, or common to it only with the Canaries. The ferns are the most conspicuous of the natural families, there being about forty species. As yet there is no published flora of the island. The following are the chief sources of information:—Bowditch's Excursions in Madeira, chapter iv. and Appendix. In the Botanical Miscellany (Hooker's), Part I. of New Series, a list by M. Frederick Höll of Dresden, with Notes by the Rev. R. T. Lowe, A. M., Cantab., and clergyman of the English Chapel at Funchal. In the Cambridge Philosophical Transactions, vol. iv. part i. ("Primitiæ Faunæ et Floræ Maderæ et Portus Sancti"), and in vol. vi. part iii. ("Novitiæ Floræ Maderensis"), many new or rare species are described by Mr Lowe. The great work on the Fishes of Madeira, since undertaken by Mr Lowe, has prevented his fulfilling the purpose, expressed in his Cambridge papers, of publishing a Flora Maderensis,—a work which, from the botanical acquirements, learning, and varied accomplishments of the author, as well as from the peculiar interest of the locality, could not fail to be one of the most valuable local floras ever published.

dula Maderensis, Gomphocarpus fruticosus, Chamæmorus coriacea, a beautiful tree belonging to the Rosaceæ, and closely allied in character and appearance to our hawthorn (*Cratægus*), *Gnaphalium crassifolium, Convolvulus althæifolius, Jasminum odoratissimum* (rare), a magnificent plant, both as regards the beauty of its dark foliage, and the colour and fragrance of its yellow flowers; and along with these the *Cassia bicapsularis, Pelargoniums,* and many introduced plants thoroughly naturalized. This short extract from the list of a day's collectanea in December, may shew what pleasure the botanist can derive from his field pursuits, even in the winter, in Madeira.

In ascending from the coast, the temperature of course diminishes with the increase of elevation. From the mountains rising almost immediately from the sea, the stations of different temperature are, in aspect and to observation, brought close together. The resulting difference in vegetation I have already described. Many other effects of this union of various climates are remarkable. In the vicissitudes of the times and seasons of the year, too, are presented some peculiar and interesting appearances. Whether spring is now anywhere on the earth to be met with in that form in which poetry loves to paint it, and tradition describes it as having reigned in the primal ages of the world, may indeed be doubted. In our own climate the season is marked by the bursting forth of fresh verdure, and the renewal of woodland melody, and all those delightful changes by which the wakening earth starts into new life and gladness; but we know nothing of the "ethereal mildness," of the "balmy softness," of the "bland zephyrs," of the spring of poesy. In those regions of the south again, where the chilling sensations and the various ungenial accompaniments of our season are absent, there is not enjoyed the pleasure arising from the sudden vicissitude in the aspect of nature that is here so striking. In Madeira, and especially in the district around Funchal, most of the elements of vernal delight are in a remarkable degree united, and a nearer approach made to the fancied perfection of the season, than in any other climate could be met with; for, while there has been throughout the winter little perceptible alteration of temperature, and the abundant verdure of the native evergreens has prevented any thing of the appearance of wintry gloom and desolation, there is yet in spring a marked and rapid change in the freshness and verdure of the landscape. The vine then puts forth its young shoots in luxuriance; and, by concealing with its verdure the trellis-work and soil on the sloping grounds, greatly adds to the beauty of the landscape. The orange, the almond, the citron, and a multitude of plants, are covered with profusion of blossom. The leaves of the oak, the plane, and other trees, elsewhere deciduous, but which here hang withered on the boughs through the winter, are displaced by the shooting of the new foliage. While this change is beginning to take place on the coast, the air being the while more genial, and the verdure as beautiful as is ever presented by an English summer, one looks up through every gradation of temperature and vegetation to bleak and barren waste, upon the snow-covered hills in the distance. Or, again, in the declining months of the year, while on the coast the summer foliage is yet unaltered, and the influence of the sun yet little diminished, there have already begun to be felt chilling colds upon the heights above the town, and the upper parts of the landscape present the variegated tints and the fading foliage of autumn. In places about 2000 feet above the sea, such as the Quinta of the Palheiro, or the villa of the Mount, the appearance is then quite like that of our northern autumnal scenery. Towards the close of the summer, in going up thither from the vineyards and orchards, and tropical vegetation of Funchal, we get amongst plantations of oak and pine, and other European trees; and the sloping grounds are clothed with heath, and broom, and furze; the note of the blackbird is heard; and the wood strawberry covers the banks; and many other sights and sounds bring recollection of scenes different from those by which we are surrounded. There is luxury even in seeing the sparkling of the dew-drops on the upland lawn, and in hearing the rustling of the falling leaves, both of which are absent on the low grounds. In going up to this place, which is within an hour's ride of the city, one might call it riding up to an English autumn, so many of the appearances and pleasures of our season are there gathered together.

On the highest parts of the island, again, all the severity of a northern winter may be presented. The cold experienced there is often extremely intense. Snow sometimes covers the hills for a considerable period, and scarcely a winter passes in which some of the inhabitants do not perish amidst storms, while crossing the mountains. On the desolate upland of the Paul de Serra such accidents are especially frequent, so that the natives regard that tract with feelings of gloomy and superstitious dread. On one occasion a party of us narrowly escaped adding another to the list of dreary legends connected with the place. We had set out from the village of St Vincente early in the morning of the 2d of April, intending to cross the Paul de Serra to the springs of Rabaçal, and to return the same evening. During the latter part of March there had been much snow on the hills; but a few days of warm sunshine had removed it so far as to enable us to cross from Funchal to the north of the island, on the previous day, without any difficulty. The weather was, however, still disagreeable, and the morning so far suspicious as to induce us to provide against being overtaken by a storm, by carrying along with us additional clothing and other comforts. The mountain summits being clear, and our time in that part of the island being then limited, we did not wish to postpone our visit to Rabaçal. The party consisted of three strangers, with our three native bourroquieros or horsemen, a carrier, and a peasant who professed to act as guide. After proceeding a short way up the beautiful valley of St Vincente, we struck off to the right by a steep road, if such a name may be given to the path by which we ascended the mountains. Accustomed as we had been to the rudeness of the island roads, and to the feats of the horses, we were yet astonished to see them achieve this rocky ascent. About two hours' riding brought us to the upper limits of the forest ground, and we entered on the more open heath. In ascending, we had encountered one or two heavy showers, but the wind drove the clouds rapidly on, so that we still had in the intervals a clear view of the valleys and villages beneath us, and the wide ocean in the distance. Gradually, however, the weather began to thicken, and, after proceeding some miles across the Serra, we were wholly enveloped in mist. We soon found need for all our spare coverings; and so piercing was the cold, that our rough pilot-coats, on the side exposed to the keen north wind, were completely cased with hoar-frost from the congelation of the mist. We had, of course, dismounted, and walked or ran, to keep up circulation and anima heat; but the depressing influence of the cold was so great as to render necessary a good deal of trouble to persuade ourselves and each other to continue our exertions. The hope of the weather clearing, together with a repugnance to returning to some friends at St Vincente without having accomplished our purpose, made us resolve, at all events, to push on till a prudent portion of the daylight had been spent in advancing. An accident, however, happening to one of the horses, determined our speedy retreat. The Portuguese, having no other clothing than their usual light dress, had suffered severely from the cold, and were so helplessly benumbed as to be able to give us little assistance. They had been speaking dolefully about "dying on the mountains," "perishing in the snow," and so forth; but we had not sooner been led to observe their sufferings, from suspecting that they were partly feigned, in order to induce us to return. We had quieted them, and prevented them leaving us, by telling them that sooner than do that, we would go on to pass the night at Seisal, or descend from the Paul by some other route. · On reflecting that the delay now experienced might involve risk if occurring later in the day, we resolved to return as speedily as possible. Any improvement in the weather was now hopeless; the miserable monotony of the mist being relieved only by occasional horrid peltings of hail and sleet. We soon, too, found that our guide had for a long time been advancing without knowing anything of the path, and that he, therefore, could be of no use in leading us along the most expeditious line of return. Our only plan was to endeavour to return as nearly as possible by the way which we had come. We had been passing over a broken stony surface, covered almost entirely with water from the melted snow; and there were no objects to note in any way the route. We therefore formed into line, leaving as much space between each as the thickness of the mist admitted, for keeping within sight and communication of each other, and in this way began anxiously to look for footsteps. On such a surface it was difficult to discover any trace; and after we had been successful, the same search had to be continued, as there was perhaps only the mark of a single hoof to direct us onward. While groping our way through the mist in this manner, we came to a piece of snow, the traces of our path across which, made us sure of our direction, and allowed us to proceed more rapidly. At length we reached a stone hut, which we had passed in the morning, and which lies at the commencement of the Campo Grande. I shall never forget the howl of joy which the Portuguese raised when they descried the house; a joy in which we participated, for if night had overtaken us, of which we had been seriously apprehensive, our situation must have been perilous. The hut, in its present state of decay, would have afforded little protection during the night, but we were glad to find shelter a while from the piercing wind; and the remainder of the contents of our basket gave us force enough to complete our journey homeward with comparative comfort. It was late in the evening before we arrived at our quarters at St Vincente, the cheerful comforts of which we enjoyed all the more, after the unexpectedly rough treatment we had met with from the elements in the April of Madeira.

But although, during the winter and spring, the weather may be thus unsteady and inhospitable among the mountains, the residents on the sea-coast almost invariably enjoy their uniform excellence of climate. In returning, for example, to Funchal, after an expedition of ten days, one of which was that which I have just described as spent upon the Paul, and the weather having been generally wet and stormy, and especially on the last day, while recrossing the mountains from St Ann's,—as we descended towards the coast, we gradually emerged from the clouds and mist into a most beautiful evening, and were surprised to learn that the weather had been fine ever since our departure. Throughout the winter, very few days indeed occur in which even the feeblest invalid need be confined to the house. To those who have, in this country, been accustomed to a climate so variable and inauspicious that a reference to the weather is the most frequent theme of passing conversation, and a succession of really fine days is a matter of surprise and common congratulation, it is delightful to look forward, with little chance of disappointment, to day after day of clear and cloudless sunshine; while every degree of temperature can be readily enjoyed on the adjacent heights; and even on the coast, the sea-breeze prevents any excess of heat from being annoying, and the rich verdure of the almost tropical vegetation affords a shelter from the direct influence of the sun. In such a climate, it is not surprising that the foot should tread lightly, and the heart beat gladly; that the approach of incipient disease should often be checked, and the constitution strengthened, so as to resist its future influence; and that alleviation of suffering and prolongation of life have here been found by many whom a rapid fate would have carried off in a less genial situation. Those who have visited Madeira have rarely been disappointed in what they had been led to expect regarding the climate, and many have been surprised to find its scenery also the finest in the world. To this many travellers have given their testimony; and certainly no combination of natural objects could be conceived more grand than some of those in the north of the island. So far as the geological structure is connected with the general aspect of the island, I should say that that which constitutes the peculiar feature in Madeiran scenery, and is the cause of its surpassing grandeur, is, that the country has all the air of rude and rugged wildness resulting from the confusion and havoc of comparatively recent volcanic action, while its scenes are at the same time on a scale of alpine magnificence. When we find, amongst this wild scenery, landscapes of a grace and variety of loveliness nowhere else surpassed, and a climate proverbially the finest in the world, we need not be surprised at the enthusiastic way in which many travellers have described the island, or at the names by which the Portuguese love to designate it, as the "Flower of the Ocean," the "Queen of the Atlantic."

On the Functions of the Colouring Matter of the Skin in the Dark Races of Mankind.

By ROBERT MORTIMER GLOVER, M.D., Lecturer on Chemistry in the Newcastle-on-Tyne School of Medicine. (Read to the British Association at the Newcastle Meeting.) Communicated by the Author.

Various hints and hypotheses have been put forth as to the functions performed by the peculiar organization of the skin in the dark races of mankind. The opinions of Sir Everard Home, published in the Philosophical Transactions for 1821, have been generally adopted by physiologists as apparently founded on a methodical attempt to investigate the subject by direct experiment, and to elucidate it by analogical reasoning. The experiments of Sir Everard give results certainly quite opposed to what has been determined by physical observers respecting the laws which affect the radiation from, and absorption of, heat by coloured surfaces. This circumstance drew my attention to the subject, and led me to repeat some of the experiments related by Sir Everard.

It may be mentioned, before entering on the subject, that this inquiry was proposed by Lord Bacon.

The structure of the skin and of its layers is yet involved in some doubt as to many particulars; but so far as our inquiry is concerned there is no doubt whatever. It is clear that there is a spongy or vascular layer between the cuticle and true skin; or on the surface of the latter, constituting a portion of it. It is also certain that the colouring matter of the skin resides in this region. And that the intensity of shade is the greater or less abundance of the colouring matter. Hence the European and the Negro furnish extreme instances in this inquiry; since in the one the colouring matter is in small quantity or of light shade, whereas the other has it so abundantly that in him we speak of the *pigmentum nigrum*. Between these extremes exist many curious varieties, in whom the functions of the colouring matter are well worthy of consideration, but we have data to reason only with regard to the European or White, and the Negro. Indeed, in many of the coloured races, the existence of something analogous to the dark pigment is only inferred, although the occurrence of Albinoes in all races should induce us to believe the presence of a pigment universal. So that what is said of the colouring matter in the Negro may be extended to all varieties of colour, reasoning by analogy.

It is scarcely possible to regard the dark colouring matter otherwise than as a provision for, in some way, enabling those who possess it in abundance to withstand the heat of the climate they inhabit. Accordingly, there are facts which prove such individuals to be more capable of withstanding the heat of torrid regions than acclimatized Europeans, or other whites born there. There are also facts to connect this power of withstanding excessive heat with the development of the dark colouring matter. Thus, Albinoes of Guinea, differing from both Europeans and their countrymen in this, that they totally want the colouring matter, according to many authors, are even less capable of resisting the heat of their native country than European strangers; indeed their skins are said to crack and blister on exposure to the sun's rays. And I am informed by Mr Granidge of Barbadoes, that he has observed the same fact in that island.

Now, when we reflect that the European cannot be without some colouring matter between the true skin and cuticle, since he must differ in this respect from the Albino, it seems as if a relation were established between the development of the pigment, and probably of the rete mucosum along with it, and the power of resisting the sun's heat in torrid regions.

It is clear that in this inquiry we should regard, not merely the physical properties of the organization we consider, nor its vital properties only, but the action and reaction of the whole, and their effect on the system of the individual. For want of a consideration of all circumstances, before the publication of Sir Everard Home's views, it was not conceived how the tint, which, on analogy, should absorb more heat than any other, could, in the hottest regions of the earth, confer any exemption on its possessor. And perhaps before this paper is concluded, it may be apparent that, since Sir Everard published, the matter has been misunderstood.

The notions entertained at present by physiologists, with regard to the operation of this pigment, are implicitly those of Sir Everard. And what they are, will appear from the following brief quotation from an elementary work :—" The secretion on the cutis vera, which gives the black colour to the skin, appears to assist in fitting men for residence in hot climates, because although such skin, by absorbing more caloric, rises to a higher temperature under the sun's rays than white skin does, yet it does not inflame so readily from a rise of temperature." Dr Alison's meaning is, that although the skin of a negro may rise to a higher temperature under the sun's rays than a white skin in the same circumstances, yet the dark skin is less likely to inflame at *that higher temperature* than the white skin at *that lower one.* This, then, is the conclusion of Sir Everard Home, whose paper I now proceed to examine.

The paper of Sir Everard Home contains alleged facts and experiments, tending to prove the Negro more capable of withstanding excessive heat of the sun's rays than the white man, and attributing this to a supposed property in dark surfaces of destroying the scorching and blistering effect of the sun's rays. The former conclusion has already been admitted. The facts by which Sir Everard supports his second position are to be considered.

Sir Everard having fallen asleep on the deck of a vessel exposed to a tropical sun, found, on awaking, his thigh scorched through a pair of *thin* white linen trousers. From this simple observation, the extravagant conclusion is drawn of black being a better protection against the sun's rays than white.

An experiment is next related, in which Sir Everard found, on exposing his hand to the sun's rays for 45 minutes, while a thermometer attached to it stood at 90°, that blisters rose and coagulated lymph was exuded. I have attempted to produce the same effect by the concentrated rays of the sun at the same temperature indicated in a similar way, and kept up to within one or two minutes of the time, when my patience was exhausted, without any result except slight reddening. Six years ago, while off the coast of Algiers, I sat for half an hour immoveable in the sun, having the greater part of my face exposed, the thermometer in the sun's rays being considerably above 100°, and though my face was scorched, nothing like the effect described by Sir Everard took place.

Sir Everard next attempted to compare the inflaming and blistering power of the sun's rays with that of hot water. He says, that water at the temperature of 120° was painful to the body, and became unbearable when still further heated. From this experiment and the preceding, he wishes us to infer a power of vesicating in the sun's rays not in proportion to their temperature.

In a third experiment, he exposed the backs of his hands to the sun with a thermometer on each, the one hand being uncovered, while the other had a covering of black cloth under which the thermometer was placed. After ten minutes, the degree of heat on each was marked, and the state of the surface examined, and this was repeated three times. During the last trial, the thermometer which had its ball covered by the cloth stood at 106°, while the other was at 98°. The exposed hand was scorched, that covered was unaffected in all the trials. I have not repeated this experiment because it is subject to an obvious fallacy, for the ball of the thermometer being between the cloth and the part, a space intervened, and across this space the heat from the cloth could only pass by radiation or by transmission through the thermometer, but not directly from the cloth to the hand, so that the heat might not accumulate on the skin.

In a fourth experiment, a Negro bore the sun's rays on his hand when a thermometer on the part indicated 100° without any scorching being the result. As the scorching of which Sir Everard speaks could be only a slight blush, it might not be observed on a sable skin. However, I do not question the result of this experiment.

Sir Everard observed in his next experiment, during the course of an eclipse, as the darkness on the sun's disk diminished, the scorching power of the rays, concentrated by a lens, increased in a ratio which is assumed to be greater than could be accounted for by the mere rise of temperature during the time of the experiment. Whence it is to be inferred that the excess of effect is due to the increased quantity of light present with the heating rays at each advance of time. A reference to the original paper will convince the reader that this assumption is established without sufficient data.

Most stress has been laid by Sir Everard, and those who have adopted his views, on the seventh experiment. We are told that, on the 9th of September, at 11 A. M., thermometer 90° in the sun, the concentrated rays applied to a piece of black kerseymere wrapped round the arm, gave no real pain, as it is expressed, during 15 minutes; and at the end of that time left no appearance on the arm; whereas, when white kerseymere was substituted, during the same time, and the concentration we are led to suppose being the same, the heat of a thermometer in the sun only 86°, yet blisters were formed. From this experiment, taken along with those preceding, it is supposed to be fully proved that although black surfaces rise to a higher temperature than white under the sun's rays, yet they scorch the surface of the body less; the scorching effect depending on a union of the rays of heat with those of light, the latter being supposed, by way of explanation, to be excluded by the black surface. First, I shall state my repetition of the experiment, and then attend to Sir Everard's explanation of his supposed fact.

I have attempted to ascertain the rise which the absorption of heat by black and white cloths respectively gives to the thermometer; to compare this observation with the effects of the same cloths under the sun's rays upon the body, and with the effect of the sun's rays on the naked skin. When the thermometer stands at about 80° in the sun, the solar rays concentrated on white cloth over the ball of a thermometer, to a space of an inch and a half in diameter by a burning-glass, caused a rise of the thermometer to 125° in a quarter of an hour. When black cloth was substituted the rise during the same period was to 172°. In five minutes, with the white, the rise was to 108°, with the black to 140°; and in some experiments in a proportion nearer that given by the longer period.

When the black and white cloths were applied to the skin at the same temperature, and with the same degree of concentration, as already mentioned, the black cloth generally caused intense pain in the course of a few minutes, and on being allowed to remain for five or at most seven minutes, produced blisters. During the same period very little apparent effect followed the application of the white cloth, though considerable pain was sometimes produced. The experiment was at different times performed on several individuals, all of whom found the black cloth give the sensation of pain sooner than the white. On the whole, I found nothing like the difference described by Sir Everard, though certainly the vesicating effect of the black surface appeared to be much greater than that of the white. From many experiments I conclude, that the rays of the sun will scorch when they are applied to the surface so as to cause a heat of about 130° and upwards. And from the experiment related by Sir Everard, it appears that hot water is capable of producing a similar effect at that temperature. From all this, I am inclined to deny the existence of a scorching power in the sun's rays, independent of the heat they contain, or at least of the effect they produce on the thermometer. Moreover, if such a power do exist, black cloth should yet scorch more than white, since it will absorb all the rays of light, whereas the other surface will reflect them.

In those experiments which I performed, care was taken to have the white and black cloth nearly of the same density. Sir Everard does not appear satisfied with his explanation of the extraordinary *fact* he relates, for he gives another furnished by Davy, who, indeed, is made to ascribe the alleged difference in vesicating power between black and white surfaces, to the former rendering the heat *sensible*. Were I not quoting from the Philosophical Transactions, a misprint might be suspected. I conclude that a black skin will absorb more heat than a white skin, and were it not for other accompanying circumstances, would produce inconvenience precisely in the ratio of the amount of heat absorbed. It must not be overlooked, however, that in the Negro the pigment is not superficial, but covered by a layer of translucent cuticle. The experiments of Dr Stark prove that colours absorb heat in proportion to their depth of hade through transparent media. It only remains to shew cuticle to be a medium in the condition of those. For urpose, I covered the balls of a differential thermometer, h cuticle, the other with cuticle of the same thickness, having ivory black rubbed on its inner surface; on bringing the thermometer into the sun's rays, the column of liquid descended rapidly in the stem, the ball of which was covered with the blackened cuticle.

It is evident, from the result of experiments which I have related, that a much less degree of heat can be borne when the heat is applied locally, or so that the perspiratory process is not excited over the whole system, than Sir Joseph Banks and others were able to bear in heated apartments where perspiration was fully excited.

This circumstance leads me to offer an explanation of the functions, or, not to speak mincingly, of the *uses* served by the peculiar colouring matter in the dark races. Blumenbach and Dr Winterbottom concur in stating the Negro to perspire more readily than the European or White, and Dr John Davy, in the 3d vol. of the Medico-Chirurgical Transactions, gives its due influence to this property. After noticing that the excessive perspiration in dark people must keep down the temperature, he proceeds, "In the inhabitants of the tropics, the exhalant arteries of the skin seem unusually expanded, and the whole apparatus peculiar to this secretion unusually developed; and I believe that the blood itself is less viscid, more fluid, and flows more readily through the vessels, so as to promote perspiration, and by that means contributing to the cooling of the surface, and being cooled itself, it contributes again when it flows back upon the heart, to the reduction of the temperature of the internal parts."

Were the inhabitant of the tropic not possessed of this organization, his system could not respond to the stimulus of heat, by a determination of fluid to the surface of the body. And the heat absorbed by the skin being prevented from entering the system by the perspiratory process, the greater radiating power of a dark skin must be beneficial in cooling.

Again, the dark skin places the Negro in the conditions of his climate by causing him to radiate heat at night, and become at that time cooler than a White in the same circumstances. This is a fact which has been observed of the Negroes. Their propensity for exercise in the open air at night has been remarked. Thus we read that when the fleet of

Hanno approached the shores of Negroland, the country which, during the day, presented only silent woods without the least trace of man, at night was lighted up with immense fires, while the woods resounded with the sounds of festivity. In a climate where, during the day, vegetation appears burnt up, the earth is cracked by the heat, and all living creatures languish; but where at night breezes refresh the air, and cheer exhausted nature, plants run with dew, and animals leave their haunts, man also, fitted by the structure of his skin to throw off heat, issues forth animated by the irresistible propensity to exercise which is always given by the bracing air of colder climates.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany.

10th September **1840.**

Elæodendron Capense.

E. *Capense*; erectum, glabrum; ramis scabro-punctatis; foliis suboppositis, petiolatis, latis, inæquilateris, coriaceis, obtusiusculis, margine subrevolutis, utrinque reticulato-venosis, ovato-oblongis, acuminatis, bicrenato-serratis, v. ovalibus et ellipticis repando-serratis, serraturis inflexo-subaculeatis, paniculis axillaribus, simplicibus, dichotomis. *Ecklon & Zeyher.*

Elæodendron Capense, *Ecklon & Zeyher*, Enumeratio Pl. Afric. Aust. 127.

DESCRIPTION.—A *tree* in the specimen described, 18 feet high, and growing freely, its trunk 4 inches in circumference near the base, and almost cylindrical for a considerable height, perfectly straight, its bark pale brown and warted, the branches spreading and pendulous. *Leaves* (2½ inches long, 1¼ broad) petiolate, subopposite, lanceolato-elliptical, the sides somewhat unequal, coriaceous, distantly spinuloso serrulate, slightly revolute in the edges, dark green above, paler below, and often becoming rusty; petiole about ⅕ of the length of the leaf, channelled above. Corymbs axillary, dichotomous, a single flower standing in the fork, and the branches supporting three flowers each; peduncle compressed. *Bracts* lanceolate, opposite, resembling greatly diminished leaves. *Flowers* minute, green. *Calyx* 4-partite, green, flat, segments oblong. *Corolla* 4-parted, twice as long as, and more delicate than, the calyx, but in all other respects similar to, and alternating with it. *Stamens* 4, opposite to the segments of the calyx, at first erect, shorter than the corolla, afterwards reflected between its segments, as well as the corolla and calyx persisting; filaments green: anthers oblong, yellow, bilobular, bursting along the face. *Germen* imbedded in a flat, green, fleshy disk, style single, shorter than the stamens, erect, stigma inconspicuous. Fruit yellow, oval, about the size of a filbert, fleshy, and containing a hard nut with 1–3 cells.

Seeds erect, compressed, almond-shaped, covered with a thick brown testa, having copious albumen and a central embryo, which is slightly curved, and passes from side to side of the greatest breadth of the seed, and from one extremity to the other.

This, notwithstanding its insignificant flowers, is a handsome evergreen, but will not endure our climate, even with the protection of a wall. We have three varieties, all free growing, differing chiefly in the breadth of the leaf and the depth of the serratures, but though growing in different degrees of heat, not, I think, varying from this cause. We are indebted for the possession of the plants to Captain Macadam, Royal Marines, who sent seeds from the Cape of Good Hope to the Royal Botanic Garden, Edinburgh, in October 1828. The plant in common cultivation as *Elæodendron Capense*, is nothing else than a narrow-leaved variety of the common Bay. How this blunder came to be made and diffused I cannot conjecture.

Grevillea dubia.

G. *dubia*, foliis ellipticis, marginibus refractis; ramis ramulisque tomentosis, floriferis racemoque abbreviato-recurvis; pistillis uncia brevioribus.—*Brown.*

Grevillea dubia, *Brown* in Linn. Trans. x. 169. *Ibid.* Prodr. i. 376. *Roem. et Schultes*, Syst. Veget. iii. 410.—Bot. Mag. 3798.

Description.—*Shrub* (5 feet high) erect, with pendulous branches, twigs covered with brownish pubescence, hairs adpressed, attached by the middle. *Leaves* elliptical or obovato-elliptical, mucronulate, spreading, having adpressed pubescence similar to that on the twigs on both sides, but silky and chiefly abundant below, lateral nerves near the margins. *Racemes* short, dense, terminal, becoming lateral and opposite to the leaves from the prolongation of the branches. *Bracts* subulate, falling very early. *Flowers* rose-coloured, geminate, on recurved pedicels, the lowest expanding first; perianth pubescent on the outside, 4-phyllous, united in the throat by a dense tuft of white wool less than half the length of the revolute limb, which on the inside is glabrous. *Stamens* small, white, sessile in the apices of the perianth. *Pistil* pedicellate, including the pedicel less than eight lines long, surrounded at its base on the lower side by a pale semilunar disk, everywhere glabrous, except at the top of the style, where it is slightly pubescent, stigma oblique, flat; germen green, obscurely furrowed above and below.

Mr Brown considers this plant scarcely specifically distinct from his *Grevillea punicea;* Roemer and Schultes repeat the doubt, and Sprengel unites them; but these writers have probably no additional information on the subject. A specimen which I received from New Holland without name, in 1824, and which I considered *G. punicea*, is distinguished from this by its leaves being broader, larger, and minutely dotted, but otherwise glabrous on the upper surface, where also the marginal nerves are less conspicuous; the raceme, too, is less dense, and the style longer. Our plant was raised at the Botanic Garden, Edinburgh, from seeds sent by Mr Cunninghame as a new species, and has flowered freely in the end of the season during several years. The figure in the Botanical Magazine is excellent.

Musa superba.

M. *superba*, subacaulis; spica nutante, bracteis cordato-ovatis, concavis, obtusis, inferioribus persistentibus; perianthii labio superiore 3-partito, lateribus revolutis; labio inferiore multo breviore, 3-lobo, lobo intermedio subulato, lateralibus multo longiore, filamentis 5, cylindraceis.

Musa superba.—*Roxb.* Coromandel, t. 223. *Ibid.* Fl. Indic. i. 667. *Idem Lib.* Ed. Car. et Wall, ii. 489. Schultes Syst. Veget. vii. 1294.

Description.—Stem scarcely any, the petioles spreading nearly from the root upwards on all sides, and forming a pseudo-stem of nine inches in diameter at the base of the specimen described. *Flower-stalk* (about 5 feet high from the ground) cernuous. *Leaves* (5 feet long, by 1 foot 7 inches broad) lanceolato-elliptical, slightly unequal at the base, of lively green on both sides, rather darker above, with a very narrow red edge, middle rib very strong, semicylindrical behind, with a deep rounded groove in front, transverse veins waved especially near the base; petioles of the lower leaves fully one-third of the length of these, and of the same shape as the middle rib, slightly clasping stem at their origin; floral leaves gradually smaller till the petioles pass into large ovate *bracts,* the lower of which only retain a small portion of the leafy expansion at the apex, but these, like the others, spread in a roseate manner, green without, red-brown within, forming, after a few only have expanded, a large elegant cernuous imbricated circular basin, of a foot in diameter, in the centre of which is the cordato-ovate mass of unexpanded bracts, surrounded by the flowers, which are half concealed among the imbricated expanded bracteæ. These are persistent, and always concave forwards, never reflexed; a few of the lower are empty, next are several with female flowers, the stamens being abortive, and then follow many, expanding in slow succession, deciduous, and covering flowers having the stamens fully developed, but with the pistil incomplete. *Perianth* single, superior, bilabiate; the upper lip (1½ inch long) coriaceous, linear, erect, revolute in the sides, reflected at the apex, ultimately 3-partite, with two slender linear internal segments laid along the fissures, the segments usually twisted together; lower lip embraced by the base of the upper, less than half its length, membranous, diaphanous, colourless, deflected, 3-lobed, the centre lobe subulate, and very slender, the lateral lobes scarcely half the length of the other, ovate, subacute, spreading. *Filaments* 5, epigynous, round, stout, erect, parallel to each other, and ranged in a row within the upper lip of the perianth. A large quantity of transparent, colourless, deliquescent jelly is discharged from the faux, between the style and the lower lip of the perianth. *Male Flower.—Anthers* twice as long as the filaments, their apices reflexed, and projecting beyond the upper lip of the perianth, bilobular, the lobes narrow, red, laid along the face of the flat linear connective towards its edges, and bursting anteriorly; pollen yellow, abundant, granules spherical. *Pistil* abortive, style subulate, equal in length to the filaments, and having a small dry stigma. *Female Flower.—Filaments* rather shorter than in the male flower, with scarcely any appearance of abortive anthers on their conical summits. *Stigma* large, white, slimy, capitate, irregularly and incompletely lobed. *Style* stout, erect, twice the length of the abortive stamens, and two-thirds of the length of the upper lip of the perianth. *Germen* angular, 3-celled. *Ovules* very numerous, globular, shortly pedicellate, their attachment being in two rows to a central placenta in each cell.

I think there cannot be any reasonable doubt that the plant I have described is the *M. superba* of Roxburgh; though the description of the size and form of the stem, as drawn by him, does not accord with our plant. His plant is described as 13 feet high; ours, though remarkably vigorous, is only 5; his has a most remarkable conical base, 7½ feet in circumference close to the ground, and 4½ immediately under the leaves; ours is scarcely 2½ feet in circumference at the ground, and scarcely tapers at all. In almost every other respect the description of Roxburgh, where it does not contradict itself, is minutely applicable to our plant, very imperfectly indeed to his figure, which also differs greatly from the specimen I now describe. It is probable that the difference in the form of the stem arises from the age of the respective plants when they flowered. The figure in the Coromandel Plants is probably taken from a plant which flowered in the Botanic Garden, Calcutta, thirty-three months after the seeds from whence it sprung were sown; our plant blossomed in the end of August 1840, fourteen months after the seed from which it sprung was put into the ground.

Every one who has visited the Botanic Garden for some years past, has been struck with the brilliant success which has attended the cultivation of the many forms of Banana under the judicious management of Mr M'Nab, and the great quantity of high-flavoured fruit which has been produced; but nothing has afforded a greater triumph than the rapid perfection of this beautiful species from imported seed, though we learn from Dr Roxburgh that it does not yield a fruit which can be eaten, but one which resembles a dry capsule, rather than a berry. We learn from the same authority, that it is a native of the valleys in the southern part of the Peninsula of India. In cultivation in the Botanic Garden, this and all the varieties of fruit-bearing Bananas have been planted in large tubs containing extremely rich soil, have had much water, and been kept in great heat. The flower bud, as I have proved by cutting down full grown plants of *Musa rosacea* and *Cavandisii*, and I think also of *M. paradisaica*, remains at the root till a time after the plant has attained its full size varying according to its treatment, and then pushes its way upwards—its appearance at the top of the stem being preceded by the evolution of one or more leaves smaller than the rest.

Orthosiphon.

Generic Character.—Calyx ovato-tubulosus, 5-dentatus, dentis superioris ovato-membranacei marginibus decurrentibus alatus, post anthesin deflexus. Corolla tubo exserto recto vel incurvo, nec gibboso, nec defracto, fauce æquali vel rarius inflata, bilabiata, labio superiore 3–4 fido, inferiore integerrimo concavo. Stamina 4, declinata. Filamenta libera, edentula. Antheræ ovato-reniformes, loculis confluentibus. Stylus apice clavato-capitatus, subinteger vel breviter emarginatus, stigmatibus in emarginatura subconfluentibus, nunc minutis, nunc incrassato-capitatis. Achenia minutissime punctulato-rugosa.

Herbæ perennes, suffruticesve. Racemi simplices, sæpius elongati, rarissime ovato-spiciformes. Verticillastri sex-flori, distantes, laxi. Folia floralia bracteæformia, ovata, acuminata, reflexa, pedicellis sæpius breviora. Pedicelli fructiferi recurvi.—*Benth.* Labiat. 25.

Orthosiphon *incurvus*, caule basi procumbente adscendente, foliis petiolatis, oblongis, crenatis, utrinque angustatis, tenuissime pubescentibus; verticillastris subsecundis; corollis villosis, incurvis, calyce triplo longioribus, fauce subæquali; staminibus corollam subæquantibus—*Benth.*

Orthosiphon incurvus.—*Benth.* Wall. Pl. As. Rar. ii. 15. *Ibid.* Labiat. 28.

Description.—Stem suffruticose, erect, branched. *Leaves* (3 inches long, $1\frac{3}{4}$ inches broad) ovate, petiolate, bright green, paler behind, rough on both surfaces, coarsely serrato-crenate, entire and wedge-shaped at the base, middle rib and oblique veins strong and very permanent behind, transverse reticulations, distinct, though much more slender. *Spike* racemose, terminal, much elongated, many-flowered unilateral; rachis furrowed, pubescent with dissimilar hairs, most of them being very short, others longer. *Bracts* ovate, acute, reflected, green, persisting. Whorls 4-flowered; flowers arising in pairs from one point, but having no common peduncle. *Pedicels* as long as the bracts. *Calyx* 10-nerved, bilabiate, the upper lip 3-nerved, rounded, reflexed, entire, mucronulate in the centre, decurrent along the sides of the tube, between which narrow wings the tube is flat above; lower lip of four slender subulate teeth, of which the lateral ones are shorter than the others and broader at the base; nearly the whole of the calyx, as well as the pedicels, has similar pubescence to that on the rachis, and is reddish-green with a pink tinge on the upper lip, which alone is glabrous. *Corolla* pale pink; tube greatly exserted, covered with dense uniform pubescence, equal to the longer hairs on the rachis, compressed laterally; dilated a little upwards, but contracted at the throat; limb bilabiate, the lower lip spoon-shaped, slightly undulate, entire, projecting forwards in a line with the lower side of the tube; the upper 3-lobed, of which the central is notched, the lateral ones being entire and reflected. *Stamens* four, didynamous; filaments glabrous, adherent along the whole of the lower side of the corolla, in whose substance they seem to be lost, free in the throat and there divaricated after shedding the pollen, and scarcely exserted; anther lobes divaricated, reddish, and applied face to face before bursting. *Pistil* intermediate in length between the longer and shorter stamens; stigma white, capitate; style curved a little upwards at the apex, glabrous, and lying with the filaments along the lower side of the tube. *Germen* of four small erect lobes, rising from a white fleshy disk, which is much enlarged on the lower side, and curved upwards, forming a large blunt, fleshy, covering to the germen, notched at the apex for the passage of the style.

This plant, native of the mountains near Silhet, was received at the Botanic Garden from the collection of his Grace the Duke of Northumberland at Syon House in October 1839, and flowered at intervals in the Stove during the whole of the following summer. Its structure is very curious, and the generic character therefore remarkably distinct.

Oxalis mandioccana.

O. *mandioccana*, caulescens, erecta; foliis simplicibus, subcordato-ovatis, acutis, subglabris, ciliatis; petiolis, pedunculisque umbellatis, compressis, subpubescentibus; pedicellis unifloris; staminibus monadelphis, interioribus pubescentibus stylos superantibus.

Oxalis mandioccana. *Raddi*, Mem. Bras. p. 21.—*DC.* Prodr. i. 696.

Oxalis impatiens ? Fl. Fluminensis, tab. 181.

Oxalis aliena, *Spreng.* Syst. Veget. ii. 423 ?—*DC.* Prodr. i. 696 ?

Description.—*Stem* shrubby, erect. *Leaves* (3 inches long, 1¾ broad) petiolate, crowded at the apices of the branches, simple, subcordato-ovate, acute, slightly bullate, glabrous and shining, ciliated, bright green in front, paler behind, and with a few short hairs on the veins; middle rib and oblique primary veins distinct, secondary veins obscure. *Petiole* half the length of the leaf, flattened, having two sharp edges placed laterally, articulated near the apex, and near the base slightly pubescent, especially on the edges. *Peduncles* precisely similar to the petioles, and not distinguishable from them, having a similar origin, crowded as they are, solitary, and not always distinctly axillary, at first deflected, afterwards erect, abrupt at the apex where the petiole is jointed, and supporting there a number of small green tooth-like erect bracteas, and several pedicels; pedicels single-flowered, at first cernuous, afterwards erect. *Calyx* 5-phyllous, of uniform pale yellowish-green colour; leaflets ovato-lanceolate, like the pedicels minutely and sparingly glanduloso-pubescent, somewhat unequal, the two innermost smaller than the others and blunt, the others subacute. *Corolla* twice as long as the calyx, white, orange-coloured on the inside near the base, cylindrical below, spreading above; petals emarginate, below cohering for a considerable way above their slender claws by the inflection of their edges, forming plates projecting internally. *Stamens* 10, monadelphous, the five outer glabrous, less than half the length of the others, which are glanduloso-pubescent in the upper half of their filaments, and equal in length to the calyx; anthers of both sets perfect, incumbent; pollen yellow. *Pistils* longer than the shorter, shorter than the longer, stamens; germens five, ovate, gradually attenuated into diverging styles, each crowned with a green capitate stigma.

This curious species was received from Dr Fischer of St Petersburg in 1839, and flowered abundantly in the stove of the Royal Botanic Garden in May 1840, and from that time to this date, producing a long succession of blossoms. It is a native of mountain woods near Rio de Janeiro. Sprengel, in his Systema Vegetabilium, quotes *Oxalis mandioccana* as a synonyme for his *O. aliena*, and Decandolle doubts whether they are different; but, if he is right in attributing to Sprengel's plant a prostrate stem, it is probable that they are. I have not seen the work of Sprengel which Decandolle quotes—Neue Entdeckungen im ganzen Umfang der Pflanzenkunde. The Syst. Veget., however, is published five years after it, and takes no notice of this character. The species must, at the least, be nearly allied to *Oxalis impatiens*,—Flora Fluminensis, tab. 181. I think it is identical with it.

Peristeria cerina, var. sordida.

P. *cerina*; scapo brevi pendulo, racemo denso, labelli lobo medio margine crispo, columna aptera.—*Lindl.* in Bot. Reg. 1953.

Var. sordida, perianthio sordide fulvo, maculato.

Description.—*Pseudo-bulb* ovate, compressed laterally, scarcely sulcate, sheathed at the base. *Leaves* (in our specimen) three from the apex, lanceolate, much attenuated at the base, with about seven nerves, which are very prominent below, furrowed above. *Scape* short, pendulous, sheathed with scales, several (in our specimen 5) flowered. *Perianth* compressed laterally, brownish-yellow, with dark circular spots, many ribbed. *Sepals* united, the upper with the lateral ones about one-third of their length, the two lateral with each other to about the middle, subacute, subequal. *Petals* elliptical, rather shorter than the sepals, and similar in colour to them. *Lip* with two ovate acute wings at the base, extending upwards along the sides of the column, where this is connected with the lateral sepals, articulated in the middle, above which it is concave, blunt and sharply crenate in the upper half of this portion, applied along the face of the column, but when fading occasionally deflected between the sepals. *Column* erect, half terete, winged along its anterior edges, toothed at its apex only. *Anther-case* rhomboid; pollen masses 2, obovate, sulcate along their outer sides; gland ovate, fleshy; stigmatic fissure narrow, transverse.

This, I think, must only be considered a variety of *Peristeria cerina*, with lurid spotted flowers. The whole structure is precisely the same, for I conceive the absence of wings to the column mentioned by Professor Lindley only means the absence of the dilatation of the wing-like edges towards their apex. Like *Peristeria pendula,* its blossoms are of short duration; and I did not at any time find it to be perfumed.

We received the plant at the Botanic Garden from Mr Knight, King's Road, Chelsea, in 1836. It flowered for the first time towards the end of March 1840.

Pithecoseris.

Generic Character. — Capitula 3–4-flora in glomerulum ovatum densè congesta, sessilia, subspicata, ebracteata. Invol. oblongi squamæ erectæ glabriusculæ acuminatæ, ext. carinatæ, int. planæ lanceolato-lineares. Receptaculum nudum. Cor. tubus extus hirsutus, lobi glabri. Achenia in iisdem capitulis difformia, alia (juniora aut sterilia?) sericeo-villosa, pappo duplici, ext. brevissimo paleaceo, int. pluriseriali setaceo, alia (matura aut fertilia?) glaberrima oblonga compressa, pappo setiformi caducissimo! Herba. Folia fere lyrata seu sinuato-pinnatifida,—*DC.* Prodr. v. 84.

Pithecoseris pacourinoides,—Martius in Herbar.—*DC.* l. c.

Description.—*Stalk* (3½ feet high) herbaceous, striated, erect, branched, round, nearly glabrous above, below covered with soft, glandular, waved, spreading, colourless hairs. *Leaves* (9 inches long, 4 broad) smaller upwards, membranous, sinuato-pinnatifid, subglabrous on both sides, much attenuated at the base, where they degenerate into the appearance of a narrowly winged petiole; semiamplexicaul and auricled, the segments projecting forwards, much reduced below the middle of the leaf, acute, inciso-lobate, the divisions mucronulate, middle rib and veins very prominent behind, the secondary veins channelled in front. *Peduncles* terminal, elongated, swelling upwards, hollow; head of flowers at first nearly flat, afterwards elongated, and densely subspicate; general involucrum awanting; partial involucrum ovato-acuminate, 3–5 flowered, the scales ovato-lanceolate, spinescento-mucronate, the outer the shorter, and subcarinate at the apex. *Receptacle* naked. *Corolla* pale lilac colour, funnel salver-shaped, the tube longer than the involucre, hairy without, limb glabrous, segments linear-lanceolate. *Stamens* exserted, the filament nearly as long as the limb of the corolla, distant, glabrous, of the same colour as the corolla, but rather darker; the anthers of still darker colour, and rather more red. *Style* twice as long as the corolla, straight, slender, of the same colour as the filaments, slightly pubescent upwards; stigma bifid, revolute. Achenia dissimilar; some abortive, clavate, pubescent; others fertile, obovate, plump, compressed, striated, glabrous and shining. Pappus deciduous; in the abortive achenia double, the exterior of one row of minute chaffs, the interior hair-like, in many rows, on the fertile achenia of the latter form only.

This curious and ornamental stove-plant, native of Pernambuco, flowered in the nursery of Mr Cunningham, Comelybank, in April 1840. I could not learn when or by whom it was introduced. It is the only known species of the genus, was discovered by Martius, and first, if I am not mistaken, described in the fifth volume of Decandolle's Prodromus.

Proceedings of the Society of Arts, Session 1839–40.

The Society of Arts held the first meeting of its Nineteenth Session in the Royal Institution, on Wednesday the 13th November, 1839.—Sir John Graham Dalyell, Kt. F.A.S., President, in the Chair.

The President, in opening the Nineteenth Session of this Society, expressed his confidence that the long recess had been prolific of invention. Animadverting on the great importance of the Arts, he shewed the indifference of mankind to their real value from being nurtured amidst their refinements. But were the season of privation to come, even of those deemed of little account, they would soon discover the worth of what they had enjoyed; they would feel how difficult it was to part with conveniences and benefits which had been in a manner incorporated with their existence. Various obstacles opposed the progress of invention and improvement of the Arts. First, the want of *encouragement;* secondly, the want of *protection.* It was the special province of the Society to obviate the former, for even the most humble devices were acceptable—as none could predict their ultimate worth;—and in regard to the latter, were the law of Patent to undergo revision, it might prove very profitable to the public.

The following communications were laid before the Society:—

1. Narrative of the Suggestions and Experiments of the late Mr James Taylor of Cumnock, in company with the late Mr Miller of Dalswinton, for the application of Steam to Navigation. Edin. 1834. Presented by Sir John Robison, K.H., Sec. R.S.E.—Sir John Robison read the statement, and exhibited the original drawings from which the first engine was made. Sir John also exhibited the first model of the paddle-wheel boat, the difficulty of working which by manual labour led Mr Taylor to propose the substitution of steam power. Sir John received the thanks of the Society, and he was requested to draw up a short abstract of the narrative, with some additional facts which he stated verbally to the Society, and not generally known to the public, in order that they might find a place in the Transactions of the Society. (666.)

2. Notice of a Polyphotal Lamp, and Reflector of Single Curvature, for steam-vessels, and other purposes. By John Scott Russell, A.M.—V.P.S.A., Greenock. This Lamp has been used for some years past with much success, on the Union, and Forth and Clyde Canals, and is about to be

introduced into steam-vessels. The reflector is so contrived as to throw a strong light forward, and to the right and left, within a certain range, and prevents the beams of light from being scattered upwards and downwards. It is thus calculated also to be used for a stationary light for pier-heads, &c. where a considerable portion of the horizon is wished to be illuminated.—The Lamp and Reflector were exhibited. (671.) Thanks voted.

3. Outline of a Plan for securing to the Manufacturers of Scotland protection against piracy of Patterns. By Mr John Whyte, pattern-drawer, Edinburgh. (669.) Referred to a Committee.

4. Mr Adie exhibited a Daguerreotype Plate of the Place de Chatelet, Paris, executed (*sans soleil*) by Vincent Chevalier. (670.) Thanks voted.

5. M. Le Sage also exhibited another French Plate. (672.) Thanks voted.

6. Donation.—The Mechanics' Magazine, Vol. XXX. 1839. Presented by the Proprietor and Publisher. (668.) Thanks voted.

7. Sir John Robison exhibited a set of Castors of a new construction, patented by Handcock. (667.) Thanks voted.

1. The following Candidates were balloted for and admitted Ordinary Members, viz:—

1. Mr Alexander Auchie, upholsterer, 25 George Street; 2. Mr Thomas Brown, Architect, 3 North Charlotte Street; 3. George T. Mitchell, M.D., 4 Fettes Row; 4. Cunningham Borthwick, Esq. Actuary, 5 North St David Street; 5. Mr John White, Pattern Drawer, 39 Clerk Street; 6. Mr Wm. Cunningham, Jeweller, 83 Lauriston Place.

2. Mr Samuel Leith, lithographer, Edinburgh, presently an Associate, was admitted an Ordinary Member, in terms of his own request, under Law II.

3. The Report of the Committee appointed to purge the Lists of Arrears was read and approved of. The Treasurer, Convener.

4. The Members received a list of the Ordinary Members of the Society as at 1st November curt.

5. The Society elected the Office-Bearers and Councillors for Session 1839–40, viz.

THE QUEEN, *Patroness.*

SIR JOHN GRAHAM DALYELL, KT. F. A. S., *President.*

DAVID MACLAGAN, M. D., F. R. S. E., } *Vice-Presidents.*
RICHARD HUNTER, Esq., H. E. I. C. }

JAMES TOD, W. S., 21 Dublin Street, *Secretary.*

MUNGO PONTON, F. R. S. E., 30 Melville Street, *Foreign Secretary.*

JOHN SCOTT MONCRIEFF, Accountant, 4 Albyn Place, *Treasurer.*

JOHN DUNN, 63 Hanover Street, *Curator of Museum.*

Ordinary Counsellors.

Professor Forbes.
W. Galbraith, A. M.
Alexander Black.
Sir J. Robison, K. H.
Gilbert L. Finlay.
John Bell, junior.
Lt.-Col. Blanshard.
David Stevenson.
Jas. Hunter, M. D.
Robert Maxton.
C. H. Wilson.
Alex. Bryson.

November 27. 1839.—David Maclagan, M. D., F. R. S. E., Vice-President, in the Chair.

1. Essay on the Form, Method of Laying, and Width of Gauge, of Rails on Railways, calculated to increase the advantages of Railway travelling—with drawings. By Mr James A. Emslie. Newcastle-on-Tyne. (665.) Referred to a Committee.

2. Specimen of Writing made with Mr E. A. Smith's (of Portsea) "Indelible Ink." Communicated by James Johnston Darling, Esq. W. S. (662.) Mr Smith to forward a bottle of his Ink, and an account of his mode of preparing it. To be submitted to the Experimental Committee.

3. Mr John St Clair's improved method of placing and tuning the additional open Strings, in his method of increasing the Tone of the Violin. The Violin, with the improved adjustment, was exhibited, and the Report of the Committee thereon was read and approved of. Sir John Graham Dalyell, Kt. Convener. (588.)

4. Donation.—Report on the various modes of Printing for the Blind. By the Rev. William Taylor of York, F. R. S., Hon. Mem. Soc. Arts 1838. From the Seventh Report of the British Association. Presented by the author. (663.) Thanks voted.

5. Donation.—Magazine for the Blind, No. I., printed in raised characters (Roman Capitals and Lowercase.) August 1839. Conducted by one of the Blind. Presented by the Rev. William Taylor, York, F. R. S. —Hon. Mem. Soc. Arts. (664.) Thanks voted.

6. Dr D. B. Reid reported to the Society what arrangements are in progress for the Exhibition of Models, processes in the Arts and Manufactures, &c. and he stated that this exhibition is to be held in the Assembly Rooms, Edinburgh, about Christmas next, the whole suit of apartments having been engaged for that purpose by the Sub-Committee. The exhibition is intended to be on a grand scale, and to be open to the public for ten or fourteen days, at a very small charge.

The following Candidates were admitted Ordinary Members, viz.—

Mr James Brown junior, 36 Drummond Place; Mr Thomas Russell, ironmonger, 7 Hunter Square.

18*th December*.—The annual General Meeting. Sir John

Graham Dalyell, F.A.S., President, in the chair. The following communications were made :—

The Report of the Prize Committee, awarding the prizes for Session 1838–9, was read; and the prizes were delivered by the President to the successful Candidates, with appropriate addresses.

In conformity with the Report of the Committee on Premiums, the following prizes were awarded :—

1. To William Davidson, M.D., Glasgow; for his "method of Decolorizing Palm Oil," and of removing the "bitter taste and lichenous odour of Iceland Moss;" both read 20th June, 1838. —*The Society's Honorary Silver Medal.* (555 and 557.)
2. To Martyn J. Roberts, Esq. of Bryn-y-caerau, Wales; for his "new method of Re-shipping a Rudder at Sea;"—read 10th April, 1839, with a Model;—printed in the Society's Transactions.—*The Society's Honorary Silver Medal.* (618.)
3. To Andrew Fyfe, M.D., F.R.S.E., V.P.S.A., lecturer on Chemistry, Edinburgh; for his Experiments on Photography, and his Demonstrations on that subject, given to the Society on 27th March, 10th and 17th April, and 3d July 1839. The results printed in the Society's Transactions.—*The Society's Honorary Silver Medal.* (632, 636, 641, and 658.)
4. To William Galbraith, A.M., C.S.A., teacher of Mathematics, Edinburgh; for his two papers on "Geodetical Surveying and Trigonometrical Levelling;"—read 14th November 1838, and 13th February 1839;—printed in the Society's Transactions.—*The Society's Honorary Silver Medal.* (586 and 613.)
5. To Edward Sang, Esq., F.R.S.E., M.S.A., Edinburgh; for his "Waving Bar" for Engravers' Ruling Machines;—exhibited 30th January, 1839;—a Description, and Drawing or Model, to be lodged.—*The Society's Honorary Silver Medal.* (615.)
6. To Messrs John Bryden and Sons, bell-hangers, Edinburgh; for a description of their "Bank-Safe Lock and Key;"—read and exhibited 29th May 1839.—*The Society's Honorary Silver Medal, and three Sovereigns for a Lock and Key on this principle, to be furnished for the Museum.* (647.)
7. To Mr Daniel Macpherson, Edinburgh; for his "new Ma-

chine for lifting Stereotype Moulds from the Types," and for his "new method of Baking the Moulds;"—read and exhibited 13th February 1839;—a Model to be furnished for the Museum.—*The Society's Silver Medal, value Ten Sovereigns.* (610.)

8. To Mr James Marshall, engineer, North Leith; for his Model, Drawing, and Description of an "Improvement in the common Smith's Vice;" read and exhibited 24th April 1839. —*The Society's Silver Medal, value Eight Sovereigns.* (637.)

9. To Mr John Gilchrist, Middle Arthur Place, Edinburgh; for his Model and Description of "Substitute for Door Springs," applied by him with much success to produce the action of self-closing on Doors of Public Offices, Lobbies, &c., and which may open either way;—read and exhibited 27th March, 1839.—*The Society's Silver Medal, value Five Sovereigns.* (631.)

10. To Mr John Whyte, pattern-drawer, Edinburgh; for his "Essay on the State of Art, as applied to British Manufactures;"—read 17th and 24th April and 15th May, 1839; and his great labour and trouble in bringing this important subject before the Society.—*Seven Sovereigns.* (601.)

11. To Mr D. T. Hope, Buccleuch Place, Edinburgh; for his trouble and drawings of a method of Increasing and Diminishing the distance of the Floats from the centre of the Common Paddle-Wheel;—exhibited 29th May, 1839.—*Three Sovereigns.* (583.)

12. To Mr John St Clair, teacher of music, Edinburgh; for a model to be furnished for the Society's Museum of his "Artificial Method of Augmenting the Tone of the Violin;"—exhibited 12th December, 1838, and in an improved form on 27th November, 1839.—*Three Sovereigns.* (588.)

13. To Mr William Bulloch, bookbinder, Edinburgh, for a model, to be furnished for the Society's Museum, of his "Moveable Stamp and Sliding Frame,"—suggested by him as a means of diminishing the expense of Embossing the Boards of Books, particularly in Cloth Binding;—exhibited 10th April 1839.—*Two Sovereigns.* (629.)

Further, while thanks were due to all who have sent communications to the Society during Session 1838–9, the special thanks of the Society were given to the following gentlemen for their respective communications, although not lodged in competition for Prizes:—

1. To Sir JOHN ROBISON, K.H., Sec. R.S.E., for his communications "on the best method of burning gas for supplying heat;"—read 13th March 1839; and "on the best method of burning gas for the purpose of illumination;"—read 27th March 1839. (581 and 582.)

2. To JOHN SCOTT RUSSELL, A.M., F.R.S.E., V.P.S.A., Virginia House, Greenock, for his paper "On the Vibration of Suspension Bridges, &c.," and suggestions for "preventing injury from this cause;"—read 16th January 1839. Printed in the Society's Transactions. (590.)

   Note.—From the great importance of this subject, the Committee humbly but strongly recommended to the Society to request Mr Russell to give them notice whenever an opportunity may occur of observing the effect, on the great scale, of a construction on the principles here laid down by him; and that, after such observation shall have been made, the subject should be again taken up by the Society.

3. To JOHN SCOTT RUSSELL, A.M., F.R.S.E., V.P.S.A., Greenock, for his Description and Drawings of a "Perfect Parallel Motion for the Land and Marine Steam-Engine;"—read 13th March, and Model exhibited 3d July 1839. (626 and 655.)

   Note.—The Committee recommended that a sum, not exceeding Ten Sovereigns, be laid out for a Model of an Engine on this principle, to be furnished by Mr Russell for the Society's Museum.

4. To JOHN ALSTON, Esq. of Rosemount, Hon. M.S.A., Hon. Treasurer to the Asylum for the Blind, Glasgow, for his "Specimens of Printing in Relief for the Blind, with Architectural Embellishments;"—read and exhibited 17th April 1839. (639.)

5. To MUNGO PONTON, Esq., W.S., F.R.S.E., Foreign Sec. S.A., for his "Notice of a cheap and simple method of preparing Paper for Photographic Drawing, in which the use of any

Salt of Silver is dispensed with;"—read, and specimens exhibited, 29th May 1839. Printed in the Society's Transactions. (654.)

The Society desired it to be understood, that all the Models to be furnished for their Museum, for which the above sums have been set apart, should be made to a Scale, and to the satisfaction of the Curator of the Museum.

The President having delivered the premiums to the successful candidates, accompanied by a suitable address, concluded with observing, "That many fair testimonies had been afforded of the progress of ingenuity, invention, and improvement of the Useful Arts, by the claims on the Society's patronage for the preceding season. Numerous valuable and interesting communications had been also made, models and drawings exhibited, though not in competition. It appeared, likewise, that the natives of Scotland were by no means of inferior genius, as might be well expected, to those of other countries; their education being excellent, their ardour and perseverance insurmountable, invested them with qualities which could not but lead to distinction and success."

The Models, Drawings, &c., of Inventions, &c. (Session 1838–9), for which the Prizes have been awarded, were exhibited.

The following communications were then read and exhibited, viz.:—

1. Part I. of a Notice on the Law of Mortality in England and Wales, as deduced from the First Report of the Registrar-General. By Edward Sang, Esq., actuary, 5 North St David Street, M.S.A. Illustrative drawings were exhibited. (676.) Part II. of this paper to be read at next meeting.

2. Notice of a Simple Copying Press. By James Hunter, M.D., Couns. S.A. The machine was exhibited, and also specimens of its performance. (675.) Thanks voted.

3. Donation.—Brief Remarks illustrative of the evils arising from Piracy of Designs. By Mr T. B. Holdway, teacher of practical drawing to the Honourable the Board of Trustees for Manufactures in Scotland. Edinburgh, 1839. Presented by the Author. (677.) Thanks voted.

4. Donation.—A Model, on a scale of one inch to a mile, illustrating the country round the Menai Straits, with a Medallic Engraving ruled from

the Model, by Freebairn, with Bate's Patent Anglyptograph. Presented by Captain Henderson, R. E. (679.) Thanks voted.

The following Candidates were admitted as Ordinary Members, viz. :—

Henry David Hill, Esq. W. S., 2 Picardy Place; William Thomas Thomson, Esq., manager of the Standard Life Assurance Company, 2 George Street; James Rolland, Esq. W. S., 14 Shandwick Place.

In terms of Law XIII. the Society appointed a committee to audit the Treasurer's books, and to report thereon, and generally on the state of the funds of the Society; the report to be given in at next meeting, viz., on 15th January 1840. The books were laid on the table.

15*th January* 1840.—David Maclagan, M. D., F. R. S. E., Vice-President, in the chair. The following communications were made:—

1. Description of an Improvement on the Mercurial Registering Thermometer of Rutherford, by which the injury frequently caused by the continued contact of the steel index with the mercury is completely prevented. By Mr John Dunn, optician, 50 Hanover Street, Cur. S.A. The instrument was exhibited. (683.) Referred to a committee.

2. Essay on Life Assurance, No. II., being on the Errors of the Bonus System. By Edward Sang, Esq., actuary, 5 North St David Street, M.S.A. (676.) The objection urged in this paper against the present system of bonus is, that it places the interests of different classes of members in opposition to each other; and that it enables the present members to declare additions to their own policies, that may have to be paid out of the funds contributed by succeeding members; and a case was mentioned in which confessedly a draft of this kind to the amount of upwards of L.126,000 was made upon the future members of a Mutual Assurance Society, and was paid by them. The objection was endeavoured to be strengthened by the remark, that, on account of the intricacy of the subject, such injustice may be committed without the smallest intention or knowledge of evil.—Essay III. to be read at next meeting.

3. Verbal Exposition of Daguerreotype. By Andrew Fyfe, M.D., F.R.S.E., M.S.A. (673.) After explaining briefly the mode of conducting the process, Dr Fyfe stated the results of experiments in which he had been engaged, with the view of illustrating its *rationale.* The golden colour given to the silver surface is, in his opinion, not a mere coating of iodine, but an *iodid of silver,* and when this is subjected to the agency of light in the camera, the affinity between the silver and iodine is weakened on those parts on which the light impinges, and hence the iodine is, as it were, loosened from the silver; consequently, when the plate is exposed to the mercurial vapours, the mercury adheres to those parts only from which the iodine has been detached; of course where

the light has not been strong, and the iodine not completely loosened, the mercurial coating is less dense. On putting the plate into the hyposulphite, the iodine is washed off, and there are then exposed two distinct metallic surfaces, a silver and a mercurial one; which, reflecting the light differently, brings out the image. That this is really the nature of the action, Dr Fyfe considered to be proved from the results of the experiments detailed by him, by which he shewed that representations similar to Daguerreotype can be procured by placing on one of the polished plates of silver without iodine, any object, such as a profile on paper, moistening the paper to make it adhere to the silver, and then mercurialising as recommended by Daguerre. In this case the mercury adheres to the metallic surface exposed, and when the paper is removed, the highly polished silver covered by it, is left resplendent, and thus gives the dark profile on the white ground. Specimens of this kind were exhibited. Similar effects are produced by exposing other plates, such as tinned iron, glass, and many others. If, when silver is used, the temperature to which the mercury is subjected be raised to about 400, the mercury adheres firmly to the silver, and accordingly the image is not apt to be rubbed off as in Daguerreotype—Experimental illustrations were exhibited. Thanks voted. Dr Fyfe was requested to give in writing a short account of his interesting experiments.

4. The large model of Mr Ponton's Galvanic Telegraph was exhibited, and the principles of its construction explained, previous to its being deposited in the Society's Museum. (682.) Thanks voted.

The following Candidates were admitted as Ordinary Members, viz.:—

1. Mr George Potts, cabinet-maker, 14 Scotland Street; 2. Thomas Thomson, Esq. W.S., 25 Melville Street; 3. Alexander Fleming, Esq. W.S., 2 South Charlotte Street.

A Committee was appointed on the subject of the Ventilation of the Hall.

29th January, 1840.—David Maclagan, M.D., F.R.S.E., Vice-President, in the chair. The following communications were made:—

1. Essay on Life Assurance, Part III. being,—On what constitute the Profits of a Mutual Assurance Society. By Edward Sang, Esq. Actuary, 5 North St David Street, M.S.A. (676.)

2. Description of a process for multiplying Photographic Drawings. By Mr C. Galpin, 1 Lowther Arcade, London. Specimens were exhibited. (680.) Referred to a Committee.

3. A Map of England and Wales, for the use of the Blind. By John Alston, Esq. of Rosemount, Honorary Treasurer to the Institution for the Blind, Glasgow, Hon. M.S.A. (684.) Referred to a Committee.

4. Donation.—The Transactions of the London Electrical Society. Part I. Comprising Papers read during 1837. Presented by Mr E. M.

Clarke, philosophical instrument-maker, Strand, London. (685.) Thanks voted.

The following Candidates were balloted for and admitted as Ordinary Members, viz.

1. Sutherland Mackenzie, Esq., 24 Charlotte Square. 2. Charles Pearson, Esq., Accountant, 59 George Street. 3. William Wood, Esq., Surgeon, 9 Darnaway Street. 4. Alexander Sprott, Esq., yr. of Garnkirk, 14 Coates Crescent. 5. Walter Calverley Trevelyan, Esq. (of Wallington, Newcastle-on-Tyne,) 108 Prince's Street. 6. Robert Christie, Esq., Accountant, 15 St Andrew Square. 7. William Wood, Esq., Accountant, 8 South Charlotte Street. 8. Thomas Sprott, Esq., W. S. 23 Rutland Square. 9. Henry George Watson, Esq., Accountant, St Andrew Square. 10. George Crosbie, Esq., National Bank, St Andrew Square. 11. Mr William Nichol, Lithographer, 27 Hanover Street. 12. James Brown, Esq., Accountant, 94 George Street. 13. James T. Murray, Esq., W.S., 31 Queen Street. 14. Andrew Tawse, Esq., W.S., 15 York Place.

John Alston, Esq. of Rosemount, Glasgow, was, at his own request, added to the list of Ordinary Members. The Society resolved that he should still hold the distinction of Honorary Member previously conferred upon him.

The Report of the Committee appointed to audit the Treasurer's Books, and to report thereon, and generally on the state of the funds of the Society, was read and approved of. Mr Horne, Convener. Thanks voted to the Committee.

The President granted discharge to the Treasurer, in terms of the above Report.

12*th February*, 1840.—Sir John Graham Dalyell, President, in the chair.

The draft of an Address by the Society to her Majesty the Queen, upon the occasion of her marriage with his Royal Highness Prince Albert of Saxe Coburg and Gotha, which had been prepared by the President at the request of a Committee of the Council, was read and unanimously approved of; and after being written out, and signed by the President, Vice-President, Secretary, and Treasurer, and sealed with the seal of the Society, was ordered to be presented in any manner the President may think proper. The Address is of the following tenor, viz.:—

"To the Queen's Most Excellent Majesty.

"Most Gracious Sovereign,

"We, the Society, under your royal patronage, instituted for the Encouragement of the Useful Arts in Scotland, humbly approach the Throne to offer our fervent congratulations on your Majesty's union with an illustrious Prince adorned by every moral virtue.

"We joyfully hail so auspicious an event, as one affording the fairest promise of contributing to your Majesty's personal felicity, and of conferring permanent benefits on the British empire.

"The public and private welfare of your Majesty—a Sovereign whose transcendent excellence has rooted the affections of her loyal people—excites in us the deepest interest, and calls forth the loudest testimonies of our anxious love, our duty, and veneration.

"We doubt not that the distinguished partner whom it has pleased your Majesty to select from among the Princes of Europe, eagerly profiting by your wise and worthy example, will emulate an equal favour for the good and virtuous; and that the same solicitude for the national prosperity, which your Majesty has ever manifested, shall be alike cherished in his breast.

"Thence do we augur a new and glorious epoch for Britain, in the praise and admiration of the world.

"We trust in the benignity of Heaven to soften your pillow, and to strew your path with flowers; that your Majesty and your Royal Consort shall reap all the happiness which an earthly condition can bestow; and that the precious fruits of your union, inheriting the virtues of their parents, shall be reserved to embellish the splendour of the Throne.

"That the Almighty Disposer of human destinies may guard your Majesty with an impenetrable shield, and that, under His Divine protection, you may be long preserved in uninterrupted security, peace, and comfort, to sway the Sceptre over this renowned and powerful Empire, is our earnest prayer.

"Signed in our name and by our appointment, by the President, Vice-Presidents, Secretary, and Treasurer, and sealed at Edinburgh the twelfth day of February, in the year one thousand eight hundred and forty.

(Signed) "JOHN GRAHAM DALYELL, *President.*
"DAVID MACLAGAN, M.D., *Vice-President.*
"RD. HUNTER, *Vice-President.*
"JAMES TOD, *Secretary.*
"JOHN SCOTT MONCRIEFF, *Treasurer.*"

The following communications were then made:—

1. Essay on Life Assurance, Part IV. being,—On the only equitable method of distributing the Profits of a Mutual Assurance Society among the Members. By Edward Sang, Esq., actuary, 5 North St David Street, M.S.A. Thanks voted. (676.) This branch of the subject of Life Assurance being now concluded by Mr Sang, a debate followed, in which Messrs Wood, More, Finlay, Fraser, and Gibson, took a part. While several of the speakers differed from Mr Sang in many of his views, they all agreed that the thanks of the Society were due to him for bringing the

subject of Life Assurance before the Society, and in particular for his exposition of the errors of the *bonus* system. It was understood that papers taking, in some respects, an opposite view of this intricate subject are to be given in and read before the Society.

2. On a new Scale for the Thermometer. By James Hunter, Esq. of Thurston, M.S.A. Thanks voted, and referred to a Committee. (688.)

3. On a new kind of Ink, obliterated with great difficulty. By Andrew Fyfe, M.D., F.R.S.E., M.S.A. Thanks voted, and referred to a Committee. (691.)

4. Donation—An Account of a Series of Experiments made with a large Magneto-Electrical Machine. By Mr E. M. Clarke, philosophical instrument-maker, Strand, London. Presented by the Author. Thanks voted. (686.)

The following Candidates were admitted as Ordinary Members, viz.:—

John Cockburn, Esq. 5 Doune Terrace; John Fraser jun. Esq. (of Standard Life Assurance Office), 23 Sciennes Street; Thomas Scott, Esq. accountant, 19 St Andrew Square; Hugh S. James, Esq. Edinburgh.

26*th February*, 1840.—Sir John Graham Dalyell, President, in the chair. The following communications were made:—

1. Notice on the use of Brass or Copper Plates, having their surfaces Silvered, for producing Pictures by the process of Daguerreotype. By Mr John Adie, optician, 58 Prince's Street, M.S.A. A specimen was exhibited. (692.) Referred to a Committee, and to be printed.

2. Model and Description of an Improved Railway Crossing. By Mr John Meiklejon, engineer, 39 Home Street, Edinburgh. (689.) Remitted to Messrs Grainger and Miller to try practically.

3. Verbal Notice of Sir W. Burnet's Process for the prevention of Dry Rot; with illustrative specimens. By Dr D. B. Reid, F.R.S.E., M.S.A. (695.) This process consists in applying a solution of Muriate of Zinc to the cloth or other substance to be preserved. It is very much cheaper than where preparations of Mercury are used; and to shew how well it acted, Dr Reid exhibited two sets of specimens, in every respect alike, of soldiers' cloth, sail cloth, ropes, &c., which were placed and kept for a year in one of the dampest cellars in Somerset House, the one set being in their natural state, and the other impregnated with the Muriate of Zinc; and while the former were quite rotten, had lost their colour, and were easily torn to fragments, the latter were as fresh in colour, and as strong in texture, as when they were put in. It is understood that a patent has been taken out to secure this valuable invention. The thanks of the Society were given to Dr Reid for exhibiting the specimens.

4. The Report of the Committee on Mr Dunn's Improvement on the Registering Thermometer was read and approved of. Dr Fyfe, Convener.

5. The Report of the Committee on Mr Galpin's Photographic process was read and approved of. Dr Fyfe, Convener.

6. The Report of the Committee on Mr Hunter of Thurston's new Scale for the Thermometer was read and approved of. Dr Fyfe, Convener.

7. Mr Sang obtained leave to print the remaining part of his Essay on Life Assurance, which is to be read at next meeting.

I. The following Candidates were admitted as Ordinary Members, viz. :—

1. John G. M. Burt, M. D., 8 Bank Street; 2. Robert Cowan, Esq. W.S., Great King Street; 3. Hugo Reid, Esq., lecturer on Chemistry, 24 Nicolson Street.

II. The Experimental Committee reported the election of their Office-bearers for 1840. Mr Ponton, Convener.

III. A letter from the Secretaries of the intended Exhibition of Arts and Manufactures at Newcastle-on-Tyne was read.

The Secretary and Curator were appointed a Committee to answer the letter, and with power to send any models, &c., which the Newcastle Committee may desire.

11th March, 1840. Sir John Graham Dalyell, President, in the Chair. The following communications were made :—

1. On the Comparative Illuminating Power of different kinds of Gas Burners. By Andrew Fyfe, M.D., F.R.S.E., M.S.A. (694.) In trying the comparative illuminating powers of different Gas-Burners, Dr Fyfe stated that he took a single Jet-burner, burning with a flame of 5 inches in length as the standard, in which case he had it so adjusted as to burn exactly one foot per hour. Assuming the light given by this burner, as thus used, to be as - - - - 100

The light given by a Fish-tail for an equal consumpt of gas is - - - - 140
By a Bat-wing, about - - - - 164
By an Argand (24 holes) - - - 180

Accordingly, for equal consumpts of gas, the additional light given, over and above that afforded by a jet, is, by the Fish-tail 40, by the Bat-wing upwards of 60, and by an Argand 80 per cent. For this purpose, however, it is necessary to use the Fish-tail and Bat-wing, burning with their full supply of gas, and to have the Argand with a flame of about three inches. On increasing the number of holes in the Argand, though the quantity of gas consumed becomes greater, the comparative illuminating power is not augmented; the increase in light being merely proportionate to the enlarged expenditure. From the numerous experiments which he had performed, Dr Fyfe stated that he had come to the conclusion, that the Argand is by far the most economical method of consuming gas when illumination is the only object, and provided, of course, so much light is required; and that the single jet is the most unprofitable, and ought never to be used. When the light of a single jet only is required, he mentioned that it is much better to have a burner with two or three holes so near each other that there shall be only one flame. When this is so used as to give the light of a jet, it consumes from 10 to 20 per cent. less of gas, 'thus causing a great saving to those who burn by meter. Thanks voted, and to be printed in the Transactions.

2. Model and Description of Apparatus for saving the lives of persons falling into any loch or standing water, on the ice giving way. By Mr Gavin Kay, Plumber and Gas-fitter, 18 Preston Street, Newington, Edinburgh. (693.) Referred to a Committee.

3. Essay on Life Assurance, Part IV.—On the money values deduced from the various Bills of Mortality. By Edward Sang, Esq. Actuary, Edinburgh. (676.)

Note.—Mr Sang obtained permission from the Society, at a former meeting, to print this part of his Essay, along with the preceding Parts, in order that those inclined to discuss the subject might have the whole fully before them.

A discussion followed upon these Essays, in which Messrs Borthwick, Wood, Finlay, and Sang took a part. Mr Wood opposed some of Mr Sang's doctrines. Thanks voted to Mr Sang, and the Essays ordered to be printed in the Transactions.

4. Remarks on a portion of an Essay read before the Society of Arts for Scotland, January 1840, on the subject of "What constitute the Profits of a Mutual Assurance Society; and on the only equitable method of dividing these among the Members." By a MEMBER of the SCOTTISH PROVIDENT INSTITUTION. Printed at London, February 1840. Communicated by ALEX. BRYSON, M.S.A. (701.)

Note.—Mr BRYSON proposed that this printed paper should be read, by permission of the Society.

The Society held that the Paper being anonymous, could not be read as an original Communication. Mr Sang then moved, that the Paper be read as part of the discussion on his Essay. Agreed to.

I. The following Candidates were elected as Ordinary Members, viz.

1. Wm. Wallace Fyfe, Esq. Writer, 11 Clarence Street: 2. Patrick Shaw, Esq. Advocate, 62 North Frederick Street; 3. David Cormack, Esq. S.S.C., 4 Pitt Street; 4. Wm. Turnbull, Esq. (of Royal Bank) 20 Great Stuart Street; 5. D. Berwick, Esq. Insurance Company of Scotland, 10 St Andrew Square.

II. Mr Alexander Rose, Lecturer on Mineralogy and Geology, Edinburgh, presently an *Associate* Member, was elected an *Ordinary Member*, in conformity with Law II.

III. The Members received the printed Annual Abstract of the Receipt and Expenditure of the Society,—and of the Funds under its management,—for the Session 1838–39.

25th March, 1840.—David Maclagan, M. D., F. R. S. E., Vice-President, in the Chair. The following communications were made:—

1. Remarks on the Profits of Life Assurance Companies, and on the Division of those Profits. By William Wood, Esq. accountant, Edinburgh, M. S. A. (703.) The subject was discussed by Mr Sang and Mr Wood. Thanks voted to Mr Wood, who was requested to print the paper.

2. Description of a Method of Drawing, by a new process, called "Drawing in Carbon." Invented by Mr C. Galpin, 1 Lowther Arcade, London. Specimens were exhibited. (681.) Thanks voted.

3. Mr Moon exhibited a plan of a Dioptric Orrery, on an entirely new principle—invented by him. (706.) Thanks voted.

4. The Report of the Committee on Mr Whyte's Outline of a Plan for protecting Patterns from Piracy, was read and approved of. Mr Whytock, Convener. A copy of Mr Whyte's Outline and of the Report, were ordered to be transmitted to Mr Emmerson Tennant, M.P., who has turned his attention very much to this subject.

5. Mr Ponton made a verbal communication relative to his having obtained a Daguerreotype impression on the lithographic stone. He was requested to give in a written notice of it, or to exhibit specimens.

6. A Letter-slit and Box for house or office doors, arranged to meet the demands of the new postage system, warning the family that the letter-carrier has been there, without detaining him, or requiring the attendance of the servant of the family, and at the same time providing for the safety of the letters, by Mr Spence, 50 Great King Street, M. S. A. The one made by Mr Dunn, for Mr Spence's door, was exhibited. (702.) Thanks voted.

7. Donation.—Remarks on M. Arago's Statements regarding the invention of the Steam Engine, in the Historical Eloge of James Watt. By Hugo Reid, Esq. M. S. A. lecturer on Chemistry, Edinburgh. Printed at Glasgow 1840. Presented by the author. (705.) Thanks voted.

8. Donation.—Description and Plates of Hearn and Davies' Patent Improved Boiler for Generating Steam, Heating Water, and other Fluids. Presented by Mr E. M. Clarke, philosophical instrument-maker, Strand, London. (687.) Thanks voted.

9. Donation.—Specimen of Maps for the Blind. Printed at the Institution at Boston, United States. Presented by John Alston, Esq. of Rosemount, Hon. M. S. A. (690.) Thanks voted.

The following Candidates were elected as Ordinary Members, viz. :—

1. Mr William Blaikie, engineer, Panmure Foundry, Edinburgh; 2. Thomas Leburn, Esq. S. S. C., 7 Teviot Row; 3. Holmes Ivory, Esq. accountant, 13 Gloucester Place; 4. Thomas G. Scott, Esq. W. S., 33 Heriot Row.

15*th April*, 1840.—William Galbraith, A.M., Counsellor, in the chair. The following communications were made :—

1. On the construction of Circular Signal Towers. By Edward Sang, Esq., M.S.A. (700.) Mr Sang promised to give a Model. Thanks voted, and the Paper ordered to be printed in the Transactions.

2. Remarks on the method proposed by Mr Sang for deducing the law of mortality in England and Wales from the mortuary registers for 1838. By William Inglis, Esq., S.S.C. 18 George Street, of Licensed Victuallers' Assurance Company. (712.)

A discussion followed, in which Mr Sang and Mr Inglis took a part. Thanks voted; and Mr Inglis was requested to print his papers.

3. Donation—On the influence of artificial light in causing impaired vision; and on some methods of preventing or lessening its injurious action on the eye, by James Hunter, M.D. (M.S.A.) surgeon to the Eye Dispensary of Edinburgh. Edinburgh, 1840. Presented by the author. (711.) Thanks voted.

4. Donation—A series of eight engraved portraits of eminent horologists, viz., Tompion, Graham, Berthoud, Harrison, Ellicot, Mudge, Arnold, and Reid. Presented by William Auld, Esq., 67 Great King Street, Edinburgh. (708.) Thanks voted.

In consequence of the time occupied in reading the previous communications, several others were postponed till next meeting.

1. The following candidate was elected as an Ordinary Member, viz.

John A. Macrae, LL.D., W.S., 25 Duke Street.

2. Motion by Robert Stevenson, Esq. "That from the commencement of next Session, the days of meeting be changed from Wednesday to Monday, at the usual hour, keeping clear, as at present, of the day on which the Royal Society hold their meetings." This motion was partly discussed, and adjourned till next meeting, to give time to consider of the proposed change.

3. In terms of Law XX. the list of prizes for Session 1840–41, as prepared by the Council, was submitted to the Society for approval. It was adopted as altered.

An answer was read from Mr Emmerson Tennent, M.P. to the Secretary's Letter sending him Mr White's outlines of a plan for preventing piracy of design, with the Society's Report thereon, acknowledging the communication as being one of great importance, and that he would lay it before the Committee of the House of Commons for their consideration.

29*th April* 1840.—Being the last *Ordinary* meeting for the Session. Sir John Graham Dalyell, President, in the chair. The following communications were made:—

1. Model and description of improvements in connection with his substitute for door-springs for doors opening either way; particularly for double doors. By Mr John Gilchrist, 14 Middle Arthur Place, Edinburgh. (713.) Referred to a committee.

2. Model, drawings, and description of the best form of rails for railways, and of the best manner of laying them. By Mr Thomas Evans, Dowlais iron-works, Merthyr Tydvil, Wales,—the patentee. (699.)—Messrs Grainger, Sang, &c., requested to give their opinion of these rails at next meeting.

3. Description and drawings of a new turret clock, in which the smallest watch, or chronometer, or a common clock, may be made to move the largest dial hands and striking hammer with the utmost accuracy, even in stormy weather; with a new apparatus for lighting the hands and dial at night, and a plan for having the clock wound up by the force of the wind. By Mr James Gall, junior, printer, Edinburgh. (717.) Thanks voted.

4. The report of the committee on Mr Gavin Kay's boat and apparatus for saving persons immersed in the water by the breaking of ice was read and approved of. Mr Crawford, Convener. (693.)

5. Donation.—The Polytechnic Journal, Vol. II. No. III. for March 1840, containing an account of the art of printing for the blind. Presented by John Alston, Esq. of Rosemount, Hon. M.S.A. (709.) Thanks voted.

6. Donations.—Specimens of printing from types in the office of Neill & Co. printers and typefounders, Old Fishmarket, Edinburgh, 1840. Presented by Messrs Neill & Co. (710.) Thanks voted.

1. The following Candidates were admitted as Ordinary Members, viz.:—

1. John Forrester, Esq. W. S., 8 Drummond Place; 2. John Macandrew, Esq. S.S.C., 28 Dublin Street.

2. On the motion of Robert Stevenson, Esq., it was resolved, almost unanimously, "That from the commencement of next

session, the day of meeting be changed from Wednesday to Monday, at the usual hour, keeping clear, as at present, of the days on which the Royal Society hold their meetings."

3. On the motion of Mr Dunn, curator of the Museum, it was resolved unanimously, "That a committee be appointed to inquire and report, whether the present arrangement for bringing out its printed Transactions should be continued, or whether some other arrangement should be substituted."—Committee appointed.

4. The list of prizes to be offered by the Society for session 1840–41, as altered according to the resolutions of last meeting, was submitted in proof, and, after some alterations, was adopted, and ordered to be printed and advertised as usual.*

13*th May* 1840.—First *Extraordinary* Meeting for this Session. Sir John Graham Dalyell, President in the Chair. The following Communications were made :—

1. On the comparative Heating Power of different kinds of Gas-burners used for Illumination ; and on the best means of burning Gas as a source of Heat. By Andrew Fyfe, M.D., F.R.S.E., M.S.A. (707.)—In a previous paper Dr Fyfe stated, as the results of his experiments on the *illuminating* power of different kinds of gas-burners, that, for equal consumpts of gas, the light given by jets being considered as 100, that by fish-tails is as about 140, by bat-wings as about 160, by argands, so constructed that as the flame must form one continued ring, about 180. From the numerous experiments on the comparative *heating* power of the same burners, he found that the heat evolved was in proportion, not to the light which they afford, but to the quantity of gas consumed, and that the same is also the case where the gas is burned on wire gauze, a practice now generally followed where gas is used as a source of heat. Accordingly, when gas is to be used for this purpose, it is of no consequence, in so far as the evolution of heat is concerned, in what manner the gas is burned, provided the combustion be complete, and provided means be adopted for preventing as much as possible the loss of heat. With regard to expense, Dr Fyfe stated that he had been able to evaporate one gallon of water by the combustion of from sixteen to eighteen feet of gas, which, at the rate gas is sold in Edinburgh, would cost twopence, or nearly so. Were coal employed for this purpose the expense would not exceed about the fourth of a farthing. Though gas is much more expensive than coal, yet where expense is not an object, it may be used advantageously, as in some processes of cooking. When the gas is burned on a gauze of about three inches in diameter, the expense of the gas on each gauze will vary from one halfpenny to a penny per hour, ac-

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* Vide p. 412.

cording to the quantity consumed. Six of these tubes in use for three hours would, therefore, cost from 9d to 1s. 6d.,—say an average about 1s. Dr Fyfe strongly recommended the use of gas for heating water baths, particularly in bed-rooms. In the trials he had made he found that a bath which held 24 gallons of water, could be raised from 50 to 100 degrees by from 17 to 18 feet of gas, at a cost, therefore, of nearly 2d. The gas was burned for this purpose by rose jet-burners attached to a tube passed under the bath, which method of consuming it he prefers to the wire gauze,—the bath was ready in three-quarters of an hour. By using burners sufficiently large, the time may be shortened to half an hour or less if required. Some remarks were afterwards made on the time required for cooking by gas, and also on the necessity of burning coal, so that the whole of the gaseous matter evolved from it shall be consumed by the due admission of air.—Thanks voted, and to be printed in the transactions.

2. Description and Drawings of his new method of Manufacturing Persian Rugs, by which a great saving in material and workmanship is effected. By Mr John Whyte, pattern-drawer, 39 Clerk Street, Edinburgh, M. S. A. Specimens of the Rugs were exhibited. (718.) Referred to a Committee.

3. There was exhibited a Face-cover for the use of Stone-cutting Masons, intended to prevent the destructive effects of inhaling the dust arising from the stone. By Mr John Bailie, 28 Cumberland Street, Edinburgh. (719.) Referred to a Committee.

4. Donation.—The Former and Present State of Glagow. By James Clelland, LL.D., President of the Glasgow and Clydesdale Statistical Society, Hon. M. S. A., &c. Glasgow, 1840. Presented by the Author. (714.) Thanks voted.

5. Donation. — Lighthouses; being the article "Sea Lights" in the Seventh Edition of the Encyclopædia Britannica. By Alan Stevenson, LL.B., F. R. S. E., civil-engineer, Edinburgh. Edinburgh 1840. Presented by the Author. (715.) Thanks voted.

6. Donation.—Observations on the application of Catadioptric Zones to Lights of the First Order in the System of Fresnel: with Tables of the Elements of Zones adapted to these Lights. By Alan Stevenson, LL.B., F.R.S.E., civil-engineer. Edinburgh 1840. Presented by the Author. (716.) Thanks voted.

I. The Members received copies of the List of Prizes offered by the Society for Session 1840–41, which they were requested to make as extensively known as possible.*

II. Motion by Robert Stevenson, Esq. for changing the day of Meeting from Wednesday to Monday, from the commencement of next Session, which was carried nearly unanimously at last Meeting, was at this Meeting carried unanimously.

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* Vide p. 412.

III. In terms of a motion by William Crawford, Esq. seconded by Claud Russell, Esq. the Society made a Donation of L.20 to the funds of the Exhibition Committee connected with the late Exhibition of Arts, Manufactures, &c. in the Assembly Rooms, in aid of the deficiency arising after paying the expenses of that interesting Exhibition.

The Exhibition Committee in return, presented to the Society the Model of Mr Ponton's Galvanic Telegraph, which had been constructed for that Exhibition.

27*th May* 1840.—Second *Extraordinary* Meeting for this Session. Sir John Graham Dalyell in the Chair. The following communications were made :—

1. Reply to Mr Wood's Remarks on the Profits of Life Assurance Societies. By Edward Sang, Esq. actuary, Edin. M.S.A. (724.)

2. Verbal Remarks on Photography with the Camera. By Andrew Fyfe, M.D., F.R.S.E., M.S.A. Specimens by Mr Talbot were exhibited. (727.) Thanks voted.

3. Remarks on Photographic Drawing. By William Fraser, Esq. Aberdeen. (722.) Thanks voted.

4. On the effects of the Curvature of Railways. By Edward Sang, Esq. (723.) Thanks voted, and to be printed in the Transactions.

5. A Specimen of Native Caoutchouc from the East Indies, the largest Sheet which has yet arrived in this country. Sent for exhibition by Mr James Dowie, bootmaker, 57 Frederick Street, M.S.A. (720.) Thanks voted.

6. Description and Drawing of an Oil Lamp for the Table, made by Messrs Smith, Blair Street, for Sir George Mackenzie of Coul, Bart., constructed on the same principle as one brought by Sir George from France. The Lamp was exhibited, by desire of Sir George Mackenzie. (725.) Thanks voted.

7. Description and Drawing of a new Method for Shutting Doors which open either way,—without the use of Springs, and requiring no additional space to what is necessary for the door itself. By Mr Daniel Macpherson, 24 Salisbury Street, Edinburgh. A Model was exhibited. (726.) Referred to a Committee.

8. By permission of the Committee of the Association for the Encouragement of the Fine Arts, Mr Tod exhibited the Globes constructed under his superintendence (on Law's principle), to be used at the distribution of the Pictures on 30th current, instead of the old octagonal wheels, as insuring by their globular form and double motion, a more perfect mixing of the tickets. The Globes and machinery constructed by Mr Kirkwood, St Andrew Street. (729.) Thanks voted.

9. A Fancy Table was exhibited, curiously made with small pieces of Straw, cut out with a penknife. Designed and executed by Lieutenant Titus Gaszynski, Polish exile, 4 Hill Place, Edinburgh. (728.) Thanks voted.

1*st July* 1840.—The Society held its last *Extraordinary* Meeting for this Session in the Royal Institution, on the 1st inst.,—Sir John Graham Dalyell, President, in the Chair,—when the following communications were made :—

1. On the formation of Life Assurance Societies in localities where the law of mortality is unknown. By Edward Sang, Esq., actuary, Edinburgh, M.S.A. (732.) Thanks voted.

2. Description of a simple Apparatus for copying letters, or other written papers. By G. F. Grahame, Esq., 31 Gilmore Place, Edinburgh. (704.) Thanks voted.

3. Letter on the subject of Protection of Patterns and Designs from Piracy,—with a copy of his observations and proposals for amending the Act 2d Vict., cap 13 and 17, 1839, submitted to the Select Committee of the House of Commons in January 1840, and copy of recommendation of his Proposals by various Manufacturers. By T. B. Holdway, Esq., 44 Hutchison Street, Glasgow. (731.) Thanks voted.

4. Opinion by Mr Sang and Mr Grainger on Mr Evan's Patent Rail for Railways. (699.) Read and approved of.

5. The Report of Committee on Mr John Adie's Daguerreotyping on Brass and Copper Plates with Silvered surfaces, was read and approved of, and thanks voted to the Committee for their very excellent Report.—Mr C. H. Wilson, Convener. (692.)

6. The Report of Committee on Mr Gilchrist's mode of opening and shutting Double Doors without springs, was read and approved of. Mr Crawfurd, Convener. (713.)

7. The Report of Committee on Mr Baillie's Face-cover for Stone-Masons, was read and approved of. Dr Douglas Maclagan, Convener. (719.)

8. The Report of Committee on Mr Macpherson's Plan for Shutting Doors without Springs, was read and approved of. Mr Crawfurd, Convener. (726.)

9. The Report of Committee on Mr Whyte's method of manufacturing Persian Rugs, was read and approved of. Mr C. H. Wilson, Convener. (718.)

10. The Report of Committee on Mr Alston's Map of England and Wales for the Blind was read and approved of. Mr W. Fraser, Convener. (684.)

11. The Report of Committee on Dr Fyfe's New Ink, of difficult obliteration, not being ready, was delayed. Mr Ponton, Convener. (691.)

12. Donation.—An Account of the Archimedes Steamer, of London, which is propelled by means of a Screw working altogether below water ;

with a Description and Plate of the Apparatus. Presented by Richard Hunter, Esq., Vice-President. (721.) Thanks voted.

13. Donation.—Notice of Experiments made in August 1839, on the comparative merits of Mr Rennie's Conoidal Propeller, and the Screw Propeller used on board of the Archimedes. Presented by Richard Hunter, Esq., V.P. (697.) Thanks voted.

14. Donation.—Notice of the Patent Trapezium or Trapezoidal Paddle-Wheel, and of the results of Mr Rennie's experiments with it and the Common Paddle-Wheel. Presented by Richard Hunter, Esq., V.P. (696.) Thanks voted.

15. Donation.—Transactions of the Society for the Encouragement of Arts, &c., London, during Session 1838-9. Vol LII. Part II. Presented by that Society. (730.) Thanks voted.

16. Donation.—Two Coloured Prints of the Archimedes Screw Steamer. Presented by F. P. Smith, Esq., the Patentee. (733.) The thanks of the Society were voted to Mr Smith, and to Captain Chappel, R.N., who commands the Archimedes, for their kindness in inviting so many of the members of the Society on board of that Steamer, in her experimental cruises in Leith Roads; and the good wishes of the Society for Mr Smith's continued and increasing success in this meritorious undertaking. The thanks were given from the Chair.

17. Memorandum by Robert Stevenson, Esq., civil-engineer, M.S.A., relative to the Archimedes Steamer, on a trip from Leith, on 30th June 1840, was read. (734.) Thanks voted.—A good deal of interesting discussion then took place on the subject of the propulsion of vessels by means of the Screw acting entirely under water, in which Mr Smith, Mr Stevenson, Mr Sang, and others, took part.

On the motion of Sir John Graham Dalyell, the President, seconded by David Maclagan, M.D., Vice-President, the Society, by acclamation, conferred upon F. P. Smith, Esq., of the Archimedes Screw Steamer, the distinction of Honorary Member, which being announced to him from the Chair, Mr Smith made an appropriate reply, in the course of which he bore testimony to the very polite attention he had received in Scotland, wherever he had touched in the course of his voyage.

In terms of Law XX. a Committee of Twelve Ordinary Members was appointed as a Prize Committee, for the purpose of awarding the Prizes for Communications made during the current Session.

The President then delivered an excellent valedictory address, and adjourned the Society to the Second Monday of November next; but with liberty to hold a meeting for Private Business in the course of the present month.

List of Prizes for Session 1840–41.

The Society for the Encouragement of the Useful Arts in Scotland, announces the following Prizes for Session 1840–41.

I. For the most important Invention, Discovery, or Improvement in the Useful Arts;—*The* KEITH MEDAL, *value Thirty Sovereigns.*

II. The Society proposes also to expend the sum of ONE HUNDRED and TWENTY SOVEREIGNS, in various Pecuniary Prizes, and Honorary Medals, in rewarding approved Communications on the following subjects,—or any other Inventions or Improvements,—or Essays or detailed Accounts of Public or other Undertakings of great National importance (not previously published), which may be submitted to them.

1. For the best series of Experiments applicable to the Useful Arts.
2. For the most important Communication of any useful Invention, Process, or Practice, from foreign countries, not yet known or adopted in Great Britain.
3. For Improvements on Instruments for measuring minute quantities of Heat, on the principle of the *Thermo-Multiplier*, or otherwise. The essentials especially to be kept in view are, Sensibility, Comparability, and Promptness of Action.
4. For a method of procuring *Small Intense* and *Constant* sources of Heat, for Philosophical purposes,—whether from obscure or luminous sources.
5. For Improvements in the Hardening of Iron, and Tempering of Steel.
6. For the best series of Experiments on Photography, particularly as to the Preserving and Multiplying of the impressions. Specimens to be deposited.
7. For the best method of obtaining a supply of Artificial Light under Water, suitable for the purposes of Divers engaged in submarine operations at considerable depths below the surface.
8. For Specimens of Lithography or Zincography, *bona fide* drawn and printed by Artists resident in Scotland. Three copies of each specimen to be lodged *on or before* 1*st March* 1841. Prizes will be given to the best and second best in competition, if approved.

General Observations.—All Communications shall be entitled to compete for the Keith Medal which comply with the terms of the announcement of that Prize, although falling under any of the above specified subjects.

The descriptions of the various inventions, &c. to be *full* and *distinct*, and, when necessary, accompanied by *Specimens, Drawings*, or *Models*.

The Society shall be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them. All Models, Drawings, &c. for which Prizes shall be given, shall be held to be the property of the Society,—the Society being in the practice of taking the value of the Model into account in fixing the amount of the Prize,—and these and all others which shall be approved of, shall be entitled to a place in the Museum.

All Communications must be written on *Foolscap* paper, leaving margins at least one inch broad on both the outer and inner sides of the page, so as to allow of their being afterwards bound up with others; and all Drawings must be on *Imperial* Drawing Paper, unless a larger sheet be requisite.

The Society reserve to themselves the power of determining whether any Communication be of sufficient merit to entitle it to the Prize for which it competes, and of modifying the amount of the Prize.

All Communications to be lodged *as soon after* 1*st November* 1840 *as possible* (except as in No. 8.), in order to insure their being read during the Session; *but those which cannot be lodged so early, will be received till* 1*st March* 1841.

Communications, Models, &c. to be addressed to James Tod, Esq. the Secretary, at the Museum of the Society of Arts, 63 Hanover Street, Edinburgh, Postage or Carriage paid.

Royal Institution, Edinburgh,
29*th April* 1840.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Mr Espy's Theory of Atmospherical Phenomena claimed by Mr Meikle of Edinburgh.*—Respecting Mr Espy's theory of atmospherical phenomena, the leading feature of which is the fall of temperature which occurs in an ascending current of air, we are requested by Mr Meikle, to state, that, in the London Quarterly Journal of

Sciences, for April 1829, and in the article Hygrometry of the Encyclopædia Britannica, vol. xii. p. 132, he has distinctly laid down the same theory in detail, and accompanied with various calculations and illustrations, which shew how it will satisfactorily account, not only for the production of clouds, mountain-caps, rain, snow, &c., but also thunder, lightning, and water-spouts, if not some of the phenomena of volcanos, and the northern lights.

GEOLOGY.

2. *Temple of Serapis.*—In a letter addressed to Professor Leonhard, from Naples, by Russegger, and dated December 1839, there is the following statement regarding the *Temple of Serapis ;*—"In respect to this temple, I differ entirely from Arago and others, who maintain that the surface on which it stands has been depressed, has remained under the sea, and has again been elevated. There is nothing either in the vicinity of the temple, or in the temple itself, which affords proofs to justify this too bold hypothesis. Every thing rather leads to the belief that the temple has remained unchanged in the position in which it was originally built, but that the sea rose, surrounded it to a height of at least twelve feet, and again retired. The elevated position of the sea continued sufficiently long to admit of the Pholas boring the pillars. This view can even be proved historically ; for the Cav. Niccolini has this year published a memoir, in which he gives the heights of the level of the sea in the Bay of Naples for a period of 1900 years, and has, with much acuteness, proved his assertions historically. The correctness of my opinion can be demonstrated and reduced to figures, by means of the dates collected by the Cav. Niccolini."

3. *Enormous Soundings at Sea.*—Captain James Ross, in a letter to the Geographical Society, gives an account of some enormous soundings made by him at sea ; one of these, 900 miles west of St Helena, extending to the depth of 5000 fathoms or 30,000 feet, the weight employed amounting to 450 lb. Another made in Lat. 33° S. and Lon. 9° W., about 300 miles west of the Cape of Good Hope, occupied 49½ minutes, in which time 2226 fathoms were sounded. These facts were thought to disprove the common opinion, that soundings could not be obtained at very great depths.

4. *Living Barnacles above the Sea-Level.*—When in the island of Elba, in the summer of 1837, I observed on rocks exposed to the mid-day sun, at the height of at least six feet above high water-mark, and in situations where they could be reached by the spray of the waves in rough weather only, numerous small living specimens of a common barnacle (*Lepas balanoides ?*). This fact may perhaps be worth recording, as likely to mislead, on a cursory examination of a foreign shore, into the opinion of there having been a recent elevation of the coast.—*W. C. Trevelyan.*

5. *Height of Tides in the Mediterranean.*—I may also take this opportunity of remarking, that very erroneous opinions appear to pre-

vail with regard to the tides of the Mediterranean. In the article on "Physical Geography," in the Encyclopædia Britannica, it is stated, that "tides are but little felt in the Mediterranean, seldom rising to six inches above the mean level," and that they are "irregular." I believe the idea is very general that *no* tide occurs in that sea, but I ascertained, by a series of observations I made at the ancient port of Antium, on the coast of the Roman states, in the summer of 1836, that the tides are *perfectly regular;* and the rise amounted there to about fourteen inches. In the more eastern parts of the Mediterranean, they are, I believe, shewn by some of the recent surveys (to which I have not had an opportunity of referring), to be much greater, though in the extreme *west* they are probably scarcely sensible, as observed in the article on Physical Geography, though the remark appears there to be erroneously applied to the whole of that sea.

In the narrow Adriatic, the tides are, of course, larger than in the Mediterranean; they appear to have been well known to the ancients, and, in more modern times, have been satisfactorily investigated by Dr Bianchi of Rimini, whose work[*] is for the number and accuracy of the observations, quite worthy of a philosopher of the present day. He proves not only the regularity of the tides in that sea, but their rise to nearly four feet (English) at new and full moon, and half that amount at neap-tides. The observations of Galileo must be familiar to many of your readers; he ascertained the difference between high and low water at Venice, to amount to as much as six or seven feet (English), and he also observed that there were tides in the Mediterranean.—*W. C. Trevelyan.*

MINERALOGY.

6. *Hydrargillite, a new mineral species.*[†]—*Description.* Colour reddish-white; occurs crystallized in crystals belonging to the rhombohedral system; the forms are the six-sided prism, feebly truncated on the lateral edges by the faces of the second six-sided prism. The faces of the first six-sided prisms are feebly longitudinally streaked, the other faces smooth. The crystals are perfectly cleavable in the direction of the terminal faces. Lustre on the terminal faces splendent and pearly, on the other faces shining and vitreous, and least on the faces of the first six-sided prism. Translucent, but transparent in thin folia. Hardness, examined by means of the file, appears less than that of calcareous spar, but the different faces vary in hardness, for the straight terminal faces are easily scratched by

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[*] "Jani Planci Ariminensis de Conchis minus notis liber. Cui accessit specimen æstus reciproci maris superi ad Littus Portumque Arimini. Editio altera, Romæ 1760," 4to. James Plancus was a name assumed by Dr Bianchi, in accordance, I believe, with the custom of the members of the Lyncæan Academy, of whom he was one of the most active, about the middle of last century.

[†] This and several of the following mineralogical notices are prepared from *Poggendorff's Annalen.*—Edit.

calcareous spar, while the lateral faces are scarcely affected by it. *Chemical Composition.*—According to Gustav Rose, it appears that this mineral consists of alumina and water, with a trace of lime, so that in a chemical view it may be arranged with Diaspore and Gibbsite, from which, however, it is distinguished by external characters. It is named Hydrargillite, from υδωρ water, and *αργιλλος* alumina. *Geognostic and Geographic Situations.*—Occurs near Slatoust in the Uralian Mountains along with magnetic iron-ore.

7. *Barsowite, a new mineral species.—Description.* Colour snow-white. Occurs massive, and in granular distinct concretions. Lustre of the granular varieties feebly pearly, the compact dull. Fracture splintery or imperfect foliated. Translucent on the edges. Hardness between that of apatite and felspar. Specific gravity = 2.740 to 2.752. Before the blowpipe it melts, but only on the edges, with difficulty; with borax it melts slowly and calmly into a transparent glass. Pounded and heated with muriatic acid, it is easily decomposed, and forms a thick jelly. *Chemical Composition.* Lime = 18.16; alumina = 32.76; silicic acid = 49.08 = 100.00. *Geognostic and Geographic Situations.*—Hitherto it has been found only in loose blocks, sometimes several cubic feet in size, in the gold sand of Barsowskoj, in the Urals. Blue crystals of corundum, greyish-black grains of zeilanite, and white folia of mica, occur imbedded in it. It is named Barsowite, from its frequent occurrence at Barsowskoj. Both externally and in chemical composition, it much resembles scapolite, but is distinguished from it by its structure, and by its relations before the blowpipe and to acids.

8. *Discovery of the repository (lagerstäte) of the Sun-Stone on the Selenga in Siberia.*—The sun-stone is a variety of felspar, which, when viewed in the direction of its chief cleavage planes, displays numberless golden spangles, which are distinctly seen in sun-light, or better by the light of a candle, while in other directions it shews only a brown colour. It occurs on the Selenga, forming, with quartz, considerable veins. The sun-stone in these veins, it is believed, may be found in masses sufficiently large to allow of their being fashioned into vases two feet high. These will be of great value, as ring-stones of this variety of felspar sell at a considerable price.

9. *Plumbiferous Aragonite.*—This curious mineral occurs in prismatic distinct concretions, intermixed with lead-glance. Its colour is greenish-grey, and it is translucent on the edges. Specific gravity = 2.977 (at 11° cent.), and 2.986 (at 13° cent.). It is a compound of 95.940 carbonate of lime; 3.859 of carbonate of lead, and water of decrepitation = 0.157 = 99.966. It occurs at Tarnowitz in Upper Silesia.

10. *Tachylite.*—This mineral, first described by Breithaupt, from specimens found near Göttingen, has since been found in the Vogelsgebirge. The following is an analysis of a specimen from the latter locality by C. G. Gmelin, silica 50.220; titanic acid 1.415; alumina 17.839; lime 8.247; soda 5.185; potash 3.866; magnesia

3.374; oxide of iron 10.266; oxide of manganese 0.397; ammoniacal water 0.497 = 101.306.

11. ***Bucklandite or Black Epidote.***—Some years ago Mr Witham of Lartington discovered in the porphyry of Glencoe a beautiful carmine red transparent variety of epidote (*Withamite* of some authors); and more lately a velvet black opaque variety, named *Bucklandite*, has been met with at the iron-mines of Arendal, also aggregated with ryakolite, among the volcanic masses of the Laacher See, and imbedded in the granite of Werchoturgi in the Urals.

12. *Chrysoberyl of the Urals.*—The same mica-slate which affords the beautiful crystals of Emerald and the Phenakite has lately been found to contain crystals and groups of crystals of chrysoberyl of considerable size, sometimes upwards of two inches in length. Gustav Rose found the crystals to have a grass-green colour, to be only translucent, and traversed by rents; so that they could not be used in jewellery. They are dichroitic: the specific gravity = 3.689, which is less than that given by Mohs, which = 3.754; the difference probably depends on the small cavities in the Uralian chrysoberyls.

13 *Discovery of considerable Veins of Strontianite in Westphalia.*—Very lately veins of this interesting, but comparatively rare mineral, have been discovered near to Hamm in Westphalia. The veins, which traverse rocks of the chalk series, are from one inch to two feet in breadth, but their other dimensions have not been ascertained. Its colour is white. It occurs in granular distinct concretions, from two to three inches in diameter; and these again are composed of scopiformly disposed prismatic concretions. Crystallized varieties are also met with. According to Professor Becks, it affords, on analysis, in 100 parts, 94.700 carbonate of strontia, 5.220 carbonate of lime, and a trace of iron and water. The carbonate of lime appears to be mechanically mixed. It is collected in hundreds of pounds, and hence these veins promise to yield strontianite in such quantity as render them important in an economical point of view.

14. ***Euxenite**, a new **Mineral** Species.*—The only known locality of this mineral is near to Jölster in northern Bergenhuus-amt in Norway. It was sent by Professor Keilhau to M. Th. Scheerer, from whose account of it in Poggendorf's Annalen. No. 5, 1840, p. 149, &c., we extract the following particulars:—*Description.* Colour brownish-black; lustre metallo-resinous; fracture imperfect conchoidal. In thin splinters it is translucent and reddish-brown, and, when pounded, exhibits the same colour, but in a feebler degree. Its specific gravity is equal to 4.60. It exhibits no traces of cleavage or crystallization. The above characters shew its close resemblance to thorite, but it is harder than that mineral, and distinctly scratches it. On careful analysis, the following result was obtained:—Tantalic acid, 49.66; titanic acid, 7.94; yttria, 25.09; oxide of uranium, 6.34; oxide of cerium, 2.18; oxide of lanthan, 0.96; lime, 2.47; magnesia, = 0.29; water, 3.97 = 98.90. It is named

euxenite from εὔξενος, on account of the many rare substances it contains. It is most nearly allied to the yttro-tantalite, yet it is distinguished from it by its specific gravity, its proportion of water, and by its containing titanic acid, cerium, and lanthan.

15. *Native Gold in Sutherlandshire.*—A rounded piece of native gold, weighing rather more than half an ounce, was found some years ago in the bed of the burn of Kildonan, a rapid mountain stream in Sutherland.—*Statistical Report of Kildonan.*

16. *Tschewkinite, a new Mineral Species.—Description.* Colour velvet-black; in thin splinters, translucent and brown; streak blackish-brown; lustre splendent and vitreous; nearly opaque; rather harder than apatite; sp. gr. = 4.508—4.549. Occurs in the Ilmengebirge near Miask, probably as a constituent of the miascite which occurs there. *Chemical Composition.*—It appears from experiments of Gustav Rose, to be principally a compound of silica, oxide of cerium, oxide of lanthan, and oxide of iron. Externally, it much resembles gadolinite, orthite, allanite, and thorite. All of them have a black colour, conchoidal fracture, shining lustre, and gelatinise with muriatic acid. We have, therefore, in the following table, placed together the characters by which they are distinguished from one another.

	GADOLINITE. (from Ytterby.)	ORTHITE.	ALLANITE. (Greenland.)	THORITE.	TYSEWKINITE.
Hardness,	6.5	6.5	6	5	5.3
Specific Gravity,	4.238	3.1	4.173	4.63	4.549
Relation to Light, . .	Pretty strongly translucent on the edges, and then it appears leek-green.	Feebly translucent on the edges, and then it appears greyish green.	Feebly translucent on edges, and then it appears brownish-green.	Very feebly translucent on the edges, and then it appears brown.	Very feebly translucent on the edges, and then it appears brown.
Colour of the Powder,	Mountain green.	Greyish-green.	Dark greyish-green.	Reddish-brown.	Blackish-brown.
Relations before the Blowpipe,	Incandesces, becomes greyish-yellow, and is not fused.	Swells and melts with frothing into a black glass.	Swells violently, and is easily melted into a black globule, which is attracted by the magnet.	Becomes brownish-red, but is not melted.	Incandesces, swells violently, becomes brown, and is melted into a black shining globule.

17. *Uranotantalite, a new Mineral Species.—Description.* Colour velvet-black; streak dark reddish-brown; occurs in imbedded flat grains, sometimes the size of a hazel-nut; lustre splendent and imperfect metallic; opaque; hardness between that of apatite and felspar; specific gravity = 5.625. *Chemical composition.*—From the experiments of Gustav Rose, it appears to be chiefly composed of tantalum and uranium, and is probably a tantalite of uranium; hence Rose names it uranotantal, according to the analogy of *yttrotantal.*

18. *Perowskite, a new Mineral Species.—Description.* Colour greyish-black and iron-black; streak greyish-white; crystallized in hexahedrons; cleavage parallel with the faces of the crystals, and

pretty perfect; lustre on the faces of the crystals, shining and metallo-adamantine; on the cleavage faces feebly shining; opaque; scratches apatite strongly, but is scratched by felspar; the hardness, therefore, is about 5.8; specific gravity = 4.017. It is infusible, *per se*, before the blowpipe. *Chemical composition.*—From the experiments of Gustav Rose, this mineral appears to contain titanium and lime. *Geognostic and geographic situations.*—It occurs along with crystallized chlorite and magnetic iron-ore, in chlorite-slate, at Achmatowsk in the vicinity of Slatoust in the Urals. *Name.*—It is named Perowskite, in honour of M. Von Perowski, an intelligent Russian mineralogist.

PHYSIOLOGY AND ZOOLOGY.

19. *On the Corpuscles of the Blood. By Dr Martin Barry.*—The author, in the course of his researches in Embryology, detailed in his "third series,"* observed that some of the corpuscles of the blood undergo progressive alterations in their structure. The corpuscles so altered he believes to be of the same kind as those described by Professor Owen; and having found that the alterations in question terminate in a separation of the corpuscles into globules, he thinks this fact confirms the idea of Professor Owen—that the blood-disc undergoes spontaneous subdivision. The author farther observed that the corpuscles of the blood, in certain altered states, undergo rapid and incessant changes of form, which cannot be traced to the action of neighbouring cilia. A corpuscle will sometimes assume the figure of an hour-glass, as if it were preparing to divide itself into two parts, but it instantaneously either regains its previous form, or assumes a new one. These motions are incessant, and so rapid that it is not easy to catch and delineate any of the resulting forms; they are compared to the writhings of an animal in pain. The author has seen them in a rabbit, as late as two hours and a half after death, and thinks it probable that they may continue for a longer time, although, when under the microscope, they gradually, and in a short time, cease; the rapid changes of form, which are at first apparent, passing into gentle undulations, and being succeeded by an alternation of rest and motion. Should these facts be thought to confirm the opinion of John Hunter, that the blood "has life within itself," or "acquires it in the act of forming organic bodies," because its corpuscles in certain states exhibit "vital actions," still his assertion that "the red globules" are the least important part of the blood will appear to have no just foundation.† The author finds that the phenomena attending what is called "vital turgescence" of the bloodvessels, depend not merely on an accumulation and stagnation of blood, but on changes in the condition of its corpuscles,

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* *Vide* page 84 of this volume of Philosophical Journal.

† Later observations, the author informs us, induce him to believe the rapid and incessant changes in the form of the corpuscles, to be caused by contiguous cilia.—EDIT.

which assume a more or less globular, or elliptical appearance resembling cells. Their interior is dark, from a great increase of red colouring matter, which accumulates around a pellucid and colourless point, corresponding in situation to that of the central part of nuclei in other cases; and so completely do the corpuscles fill their vessels, that the fluid portion of the blood is excluded, and the corpuscles are compressed into polyhedral forms. This condition of the blood-corpuscles during vital turgescence of the vessels the author thinks deserving of consideration, in connexion with many of the phenomena attending local accumulations of blood, both in health and in disease; and more especially with reference to increased pulsation, the exudation of colourless fluid, and the heat and redness of inflamed parts. According to the views of the author, the formation and nourishment of organs is not effected merely by the fluid portion of the blood, for he has discovered that the cells which he shewed in his "Third Series of Researches in Embryology" form the chorion, are altered blood-corpuscles; and he has farther found that muscular fibre (that is, the future muscle-cylinder, not the fibril) is formed by the coalescence of cells, which also are derived from corpuscles of the blood. He has seen and figured every stage of transition, from the unaltered blood-corpuscle to the branched cells forming the chorion, on the one hand, and to the elliptical or oblong muscle-cells, on the other. The colour is not changed, except that the bood-corpuscles, when passing into cells for the formation of muscle, become of a much deeper red. There seems to occur in these an increase of red colouring matter. Valentin, in describing the mode of the formation of muscle, had stated that globules approach one another and coalesce to form threads, which in many places have the appearance of a necklace, but subsequently lose the traces of division, and become cylinders. Schwann had conjectured that the globules just referred to—as having been observed by Valentin—are cells, and that these cells coalesce to form a secondary cell, that is, the muscle-cylinder. The author confirms the observations of Valentin and the conjectures of Schwann, with the addition, that the globules coalescing to form the muscle-cylinder are blood-corpuscles which have become cells. The fibrils appear to be subsequently formed within the cylinder, which thus becomes the muscular fasciculus. The medullary portion of the cylinder appears to be composed of the pellucid objects, one of which is contained within each altered blood-corpuscle. Some of these pellucid objects, however, continue to occupy a peripheral situation. The author thinks it is not probable that muscular fibre and the chorion are the only tissues formed by the corpuscles of the blood; he is disposed rather to inquire, how many are the tissues which they do not form? Nerves, for instance, are known to arise very much in the same manner as muscle-cylinders; and epithelium-cells sometimes present appearances which have almost suggested to the author the idea that they were altered corpuscles of the blood. Schwann had previously shewn, that "for all the elementary parts of organisms there is a common principle of development,"—the elementary parts of tissues having a like origin in cells, however different the functions of those tissues. The facts made known in the present memoir, not only afford evidence of the justness of the views of Schwann, but they farther shew that objects, such as the corpuscles of the blood, having all the same appearance, enter immediately into the formation of tissues which physiologically are extremely different. Some of these corpuscles arrange themselves into muscle, and others become metamorphosed into constituent parts of the chorion. But the author thinks it is not more difficult to conceive corpuscles having the same colour, form, and general appearance, undergoing transformations for very different purposes, than to admit the fact made known by two of his preceding memoirs, —namely, that the nucleus of a cell, having a central situation in the group which constitutes the germ, is developed into the whole embryo, while the nuclei of cells occupying less central situations in the group, form no more than a minute portion of the amnion. It is known that in the bee-hive a grub is taken—for a special purpose —from among those born as workers, which it perfectly resembles until nourished with peculiar food, when its development takes a different course from that of every other individual in the hive.—*Proceedings of the Royal Society of London.*

20. *Researches in Embryology. Third Series.—Additional Observations. By Dr Martin Barry.*—Having, in the paper to which the present is supplementary, made known the fact that the germinal spot in the mammiferous ovum resolves itself into cells, with which the germinal vesicle becomes filled, the author has since directed his attention to the corresponding parts in the ova of birds, batrachian reptiles, and osseous fishes, which he finds to be the seat of precisely the same changes. The numerous spots in the germinal vesicle of batrachian reptiles and osseous fishes are no other than the nuclei of cells. The cells themselves, from their transparency, are at first not easily discerned, and appear to have hitherto escaped notice ; but after the observer has become aware of their presence, they are, in many instances, seen to be arranged in the same manner, and to present the same interior themselves as the corresponding cells in the ovum of mammalia. In the representations given by Professor Rudolph Wagner, the discoverer of the germinal spot, the author recognises evidence of the same changes in ova throughout the animal kingdom. He confirms and explains the observations of R. Wagner, that in the ova of certain animals an originally single spot divides into many, and that in the ova of other animals the number of spots increases as the ovum ripens. But he expresses also the opinion, that in all ova there is originally but a single spot, this being the nucleus of the germinal vesicle or cell. The analogy between the ova of mammalia and the animal above mentioned, extends also to the substance surrounding the germinal vesicle, which consists of nucleated cells.

21. ***Form of the Blood-particles of the Ornithorhynchus hystrix.***

By Dr John Davy.—A portion of the blood of the *Ornithorhynchus hystrix*, mixed when fresh with a strong solution of common salt, being examined by the author, exhibited a few globules of irregular shape. Another portion, preserved in syrup, contained numerous globules, most of which had an irregular form, but many were circular; none, however, were elliptical, like those of birds. Hence the author concludes, that in form they accord more with those of Mammalia.—*Proceedings of the Royal Society of London.*

22. *On the Minute Structure and Movements of Voluntary Muscles. By W. Bowman, Esq., Demonstrator of Anatomy in King's College, London, &c.*—The objects of the author, in this paper, are the following,—1*st*, To confirm, under some modifications, the view taken of the primitive fasciculi of voluntary muscles being composed of a solid bundle of fibrillæ. 2*dly*, To describe new parts entering into their composition: and, 3*dly*, To detail new observations on the mechanism of voluntary motion. He first shews that the primitive fasciculi are not cylindrical, but polygonal threads; their sides being more or less flattened where they are in contact with one another; he next records, in a tabular form, the results of his examination of their size in the different divisions of the animal kingdom. It appears that the largest are met with in fish; they are smaller in reptiles, and their size continues to diminish in insects, in mammalia, and lastly, in birds, where they are the smallest of all. In all these instances, however, an extensive range of size is observable, not only in different species, but in the same animal, and even in the same muscle. He then shews that all the fibrillæ into which a primitive fasciculus may be split, are marked by alternate dark and light points, and that fibrillæ of this description exist throughout the whole thickness of the fasciculus; that the apposition of the segments of contiguous fibrillæ, so marked, must form transverse striæ, and that such transverse striæ do in fact exist throughout the whole interior of the fasciculus. He next inquires into the form of the segments composing the fibrillæ, and shews that their longitudinal adhesion constitutes *fibrillæ*, and their lateral adhesion *discs*, or plates, transverse to the length of the fasciculus; each disc being, therefore, composed of a single segment from every one of the fibrillæ. He shews that these dics always exist quite as unequivocally as the fibrillæ, and gives several examples and figures of a natural cleavage of the fasciculus into such discs. It follows that the transverse striæ are the edges, or focal sections of these discs. Several varieties in the striæ are then detailed, and the fact noticed that in all animals there is frequently more or less diversity in the number of striæ in a given space, not only on contiguous fasciculi, but also on the same fasciculus at different parts. The author then proceeds to describe a tubular membranaceous sheath, of the most exquisite delicacy and transparency, investing each fasciculus from end to end, and isolating it from all other parts; this sheath he terms *Sarcolemma*. Its existence and properties are shewn by several modes of demonstration; and among others, by a

specimen in which it is seen filled with parasitic worms (Trichinæ), which have removed all the fibrillæ. The adhesion of this sarcolemma to the outermost fibrillæ is explained. It is also shewn that there exist in all voluntary muscles a number of minute *corpuscles* of definite form, which appear to be identical with, or at least analogous to, the nuclei of the cells from which the development of the fasciculi has originally proceeded. These are shewn to be analogous to similar bodies in the muscles of organic life, and in other organic structures. The author next describes his observations on the mode of union between tendon and muscle; that is, on the extremities of the primitive fasciculi. He shews that in fish and insects the tendinous fibrillæ become sometimes directly continuous with the extremities of the fasciculi, which are not taper, but have a perfect terminal disc. In other cases the extremities are shewn to be obliquely truncated, where the fasciculi are attached to surfaces not at right angles to their direction. Lastly, he states his opinion, and gives new facts on which it is founded, that in muscular contraction the discs of the fasciculi become approximated, flattened, and expanded: the fasciculi, of course, at the same time becoming shorter and thicker. He considers that in all contractions these phenomena occur; and he adduces arguments to shew the improbability of the existence of any rugæ or zigzags as a condition of contracting fasciculi in the living body. The paper is abundantly illustrated by drawings of microscopic appearances.—*Proceedings of the Royal Society of London.*

23. *Abundance of Wild Swans in the Highland Lochs does not necessarily indicate a severe Winter in Iceland and Faroe.*—In the notice of the proceedings of the Wernerian Society in No. 56 of your Journal, p. 411, I find it was stated at the meeting of December 21st, that wild swans were abundant in the Highland Lochs, "indicating a severe winter in Iceland and Faroe." That this is not necessarily indicated by the presence of an abundance of swans in this country, the following extract from a letter I last month received from Faroe will shew: my friend Mr Schroter, who has resided there many years, says, "The winter has been without frost and in every respect the best winter I or any body now living can remember. From the Solstice in December till the Equinox (vernal) we had no storm, very little snow, and less frost; especially from January 28. till March 23., it was surprising how dry the weather was without frost. You may imagine it from this, that in many places the turf moor was cracked into prismatic divisions, and some peats I had by chance dug up February 11. were quite dry March 17., so that I could use them for fuel. I never heard of that before; the frost commonly spoils the pits in May, often in June, but there was no frost to be seen on the pits in the heart of this winter."—*W. C. Trevelyan.*

24. *Rare Zoophytes on the Coast of Arran.*—We lately picked up on the beach at Brodick in Arran, a specimen of the beautiful *Goniaster Templetoni*, and of the *Luidia fragilissima* of Forbes in Werne-

rian Transactions; the specimen of the latter fortunately did not shew its fragile tendency, so that I have been able to preserve it entire. We have also found numerous specimens of the very beautiful and curious *Actinia maculata* of Johnston's British Zoophytes, and always accompanied by the hermit crab, inhabiting the shell and the horny extension of it, to which the Actinia is attached.—*W. C. Trevelyan.*

NEW PUBLICATIONS.

1. *Illustrations of the Zoology of Southern Africa.* No. XI. By Dr Smith. Smith, Elder, & Co., London.

The first plate of this number represents the *Gerbillus auricularis*, a new species, found in the western districts of Southern Africa, principally north of the Orange river. The description is given with the author's usual ability; it harmonizes with the coloured figures in the plate, a merit of rather a rare occurrence, even in some works on Natural History of high pretensions. Six species of the *Petrel Family* are beautifully represented and well described: these are, of the genus *Procellaria*, the following species—*P. glacialoides*, *P. macroptera*, *P. Forsterii*, *P. turtur*; of the genus *Pachyptila*, the following—*P. Banksii*; of the genus *Puffinus*, the *P. cinereus*; of the beautiful genus *Cinnyris*, one species, viz. *C. verroxi*, is figured and described. Of Fishes, the following are figured and described, 1. *Elops Capensis* of Smith, also *Bagrus Capensis* of Smith.

2. *Journal of the Asiatic Society of Bengal.* Edited by the Acting Secretaries. Year 1839.

Number for September contains—1. *On Fifteen Varieties of Fossil Shells found in the Saugor and Nerbudda Territories.* By George G. Spilsbury, Esq. Surgeon.—2. *Note on the River Goomtee with a Section of its Bed.* By V. Tregear, Esq. Jounpore.—3. *Memoranda relative to Experiments on the Communication of Telegraphic Signals induced by Electricity.* By W. B. O'Shaughnessy, Esq. Assistant-Surgeon, Professor of Chemistry, Medical College, Calcutta, and officiating Joint-Secretary to the Asiatic Society of Bengal.—4. *Extract from a Memoir on the Preparations of the Indian Hemp, or Gunjah* (Cannabis Indica); *their effects on the Animal System in health, and their utility in the Treatment of Tetanus, and other convulsive Diseases.* By Dr W. B. O'Shaughnessy.—5. *Memoir on the Climate, Soil, Produce, and Husbandry of Afghanistan, and the neighbouring Countries.* By Lieut. Irwin.—6. *Meteorological Register.*

Number for October contains—1. Continuation of the *account of Afghanistan.* By Lieut. Irwin.—2. *March between Inhow and Sangor,* 1839. —3. *On an Aerolite presented to the Society.*—4. *Extracts from the Mohit* (the Ocean), *a Turkish Work on Navigation in the Indian Seas.* Translated and communicated by I. Von Hammere, Baron Purgestall, &c.—5. *Description of an Astronomical Instrument, presented by Rajah Ram Sing, of Khota, to the Government of India.* By T. J. Middleton, Esq. of the Hindoo College, Calcutta.—6. Continuation of Assistant-Surgeon O'Shaughnessy *on Indian Hemp.*—7. *On the Ex-*

plosion of Gunpowder under Water by the Galvanic Battery, &c. By Assistant-Surgeon O'Shaughnessy.—8. *Proceedings of the Asiatic Society.*—9. *Meteorological Journal.*

Number for November contains—1. Continuation of Lieut. Irwin's *Memoir on Afghanistan.*—2. *Journal of a Trip through Kunawur Hungrung, and Spiti, undertaken in the year* 1838, *under the patronage of the Asiatic Society of Bengal, for the purpose of determining the Geological Formation of those Districts.* By Thomas Hutton, Lieut. 37th Regiment, No. 1. Assistant-Surveyor of the Agra Division.—3. *Notes on various Fossil Sites in the Nerbudda ; illustrated by Specimens and Drawings.*—4. *Proceedings of the Asiatic Society.*—5. *Meteorological Register.*

3. *Madras Journal of Literature and Science*, **published under the auspices of the Madras Literary Society, and the Auxiliary Royal Asiatic Society. Edited by the Secretaries of the Asiatic Department. Number from July to September 1839.**

The following are the papers in this number:—1. *Report on the Mackenzie MSS.*—2. *Essay on the Language and Literature of the Telugus.* By Charles P. Brown, Esq., of the Madras Civil Service.—3. *Catalogue of the Birds of the Peninsula of India, arranged according to the Modern System of Classification ; with brief Notes on their habits and geographical distribution, and description of new, doubtful, and imperfectly described species.* By T. C. Jerdon, Assistant-Surgeon, 2d Madras Light Cavalry.—4. *A Catalogue of the Species of Mammalia found in the Southern Mahratta Country ; with their Synonymes in the native languages in use there.* By Walter Elliot, Esq., Madras Civil Service.—5. *Some account, Historical, Geographical, and Statistical, of the Ceded Districts.* By Lieutenant Newbold, A. D. C. to General Wilson, &c.—6. *Journey of the Russian Mission from Orenbourg to Bokhara.* Translated by Colonel Monteith, K. L. S., Chief Engineer of the Madras Army.—7. *Report on the Manufacture of Tea, and the extent and produce of the Tea Plantations of Assam.* By C. A. Bruce, Esq.. Literary and Scientific Intelligence.—8. *Horary Meteorological Observations, made agreeably to the suggestions of Sir J. Herschel.* By T. G. Taylor, Esq. H. E. I. C. astronomer.—9. *Horary Meteorological Observations made at the Summer Solstice* 1839, *at the Trevandrum Observatory.* By G. Sperschneider, Superintendent.—10. *Meteorological Journal kept at the Madras Observatory.*

List of Patents granted for Scotland from 25*th June to* 17*th September* 1840.

1. To William Neale Clay of Flimby, in the county of Cumberland, gentleman, "certain improvements in the manufacture of iron."—25th June 1840.

2. To Rice Harris of Birmingham, in the county of Warwick, gentleman, "certain improvements in cylinders, plates, and blocks used in printing and embossing."—25th June 1840.

3. To Robert Cook of Johnston, in Renfrewshire, engineer and millwright, "for the making of bricks by machinery, to be wrought either by steam or other power."—30th June 1840.

4. To John Hemming of North Bank, Regent's Park, in the county of Middlesex, gentleman, "improvements in gas meters."—30th June 1840.

5. To Thomas Richardson, of the town, and county of the town, of Newcastle-upon-Tyne, chemist, "a preparation of sulphate of lead, applicable to some of the purposes for which carbonate of lead is now applied."—30th June 1840.

6. To David Morrison of Wilson Street, Finsbury, in the county of Middlesex, ink-maker, "improvements in printing."—30th June 1840.

7. To Jonathan Sparke of Langley Mills, Northumberland, agent, "certain improved processes or operations for smelting lead-ores."—2d July 1840.

8. To William M'Murray, of Kinleith Mill, near Edinburgh, paper-maker, "certain improvements in the manufacture of paper."—2d July 1840.

9. To Robert Stirling Newall of Dundee, in the county of Forfar in Scotland, being partly a communication from abroad, and partly by invention of his own, "certain improvements in wire ropes, and in machinery for making such ropes, which ropes are applicable to various purposes."—2d July 1840.

10. To Charles Greenway of Douglas, in the Isle of Man, Esquire, "certain improvements in reducing friction in wheels of carriages, which improvements are also applicable to bearings and journals of machinery."—2d July 1840.

11. To John Lothian of Edinburgh, geographer, "improvements in apparatus for measuring or ascertaining weights, strains, or pressure."—7th July.

12. To John Swain Worth of Manchester, in the county of Lancaster, merchant, being a communication from abroad, "improvements in rotatory engines to be worked by steam and other fluids, such engines being also applicable for pumping water and other liquids."—7th July 1840.

13. To Thomas Peet of Bread Street, Cheapside, in the city of London, gentleman, being a communication from abroad, "certain improvements in steam-engines."—10th July 1840.

14. To Edward Thomas Bainbridge of Park Place, St James, in the county of Middlesex, Esquire, "improvements in obtaining power."—10th July 1840.

15. To John Juckes, of Shropshire, gentleman, "improvements in furnaces or fire-places, for the better consuming of fuel."—10th July 1840.

16. To James Harvey of Basing Place, Waterloo Road, in the county of Surrey, timber-merchant, "certain improvements in paving streets, roads and ways, with blocks of wood, and in the machinery or apparatus for cutting or forming such blocks."—13th July 1840.

17. To William Henry Bailey Webster of Ipswich, in the county of Suffolk, surgeon in the Royal Navy, "improvements in preparing skins and other animal matters for the purposes of tanning, and the manufacture of gelatine."—13th July 1840.

18. To Alexander Bow of Crown Street, Hutchesontown, Glasgow, in the county of Lanark, Scotland, builder, "improvements in furnaces and flues by the introduction and application of hot air thereto, and for the consumption of smoke and economising fuel."—14th July 1840.

19. To Christopher Nickels, of the York Road, Lambeth, in the county of Surrey, gentleman, being a communication from abroad, "improvements in the manufacture of braids and plats."—23d July 1840.

20. To William Palmer of Sutton Street, Clerkenwell, in the county of Middlesex, candlemaker, "improvements in the manufacture of candles, and in apparatus for applying light."—23d July 1840.

21. To Daniel Gooch of Paddington Green, in the county of Middlesex, engineer, "improvements in wheels and locomotive engines to be used on railways."—24th July 1840.

22. To Henry Dircks of Liverpool, in the county of Lancaster, engineer, "certain improvements in the construction of locomotive steam-en-

gines, and in wheels to be used on rail and other ways, parts of which improvements are applicable to steam-engines generally."—24th July 1840.

23. To Joseph Tunnicliff of Charles Street, in the City Road and county of Middlesex, engineer, "certain improvements in the machinery or process for the reduction or comminution of dye woods, for facilitating the extraction of their colouring matter."—27th July 1840.

24. To John George Bodmer of Manchester, in the county of Lancaster, engineer, of an extension of seven years from 18th August 1824, of a patent granted to him for "certain improvements in the machinery for cleaning, carding, drawing, roving, and spinning of cotton and wool."—27th July 1840.

25. To Richard Smith and Richard Hacking both of Bury, in the county of Lancaster, machine-makers, "certain improvements in machinery, for spinning cotton and other fibrous substances."—28th July 1840.

26. To Richard Smith and Richard Hacking both of Bury, in the county of Lancaster, machine-makers, "certain improvements in machinery or apparatus for drawing, slubbing, roving, and spinning cotton, wool, flax, silk, and other fibrous substances."—30th July 1840.

27. To John Aitchison of Glasgow, in Scotland, at present residing at No. 144 Minories, in the city of London, merchant, and Archibald Hastie of West Street, Finsbury Square, in the county of Middlesex, merchant, "certain improvements in generating and condensing, heating, cooling, and evaporating fluids."—31st July 1840.

28. To Richard Beard of Egremont Place, New Road, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in apparatus for taking or obtaining likenesses and representations of nature and of drawings, and other objects."—4th August 1840.

29. To Richard Hodgson of Salisbury Street, Strand, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the forms or shapes of materials and substances used for building and paving, and in their combination for such purposes."—4th August 1840.

30. To John Rapson of Park Street, Park Place, Limehouse, in the county of Middlesex, engineer, "improvements in steering ships and vessels."—4th August 1840.

31. To Thomas Oram of Lewisham, in the county of Kent, gentleman, "improvements in the manufacture of fuel."—4th August 1840.

32. To Samuel Lawson of Leeds, in the county of York, and John Lawson of the same place, engineers and co-partners, being a communication from abroad, "improvements in machinery for spinning, doubling, and twisting flax, hemp, wool, silk, cotton, and other fibrous substances."—6th August 1840.

33. To George Clarke of Manchester, in the county of Lancaster, manufacturer, "certain improvements in the construction of looms for weaving."—6th August 1840.

34. To Robert Hampson of Mayfield Print-Works in Manchester, in the county of Lancaster, calico-printer, "an improved method of block-printing on woven fabrics of cotton, linen, silk, or woollen, or of any two or more of them intermixed, with improved machinery, apparatus, and implements for that purpose."—13th August 1840.

35. To Colin Macrae of Cornhill, Perthshire, Scotland, gentleman, being a communication from abroad, "improvements in rotatory engines worked by steam, smoke, gases, or heated air, and in the modes of applying such engines to useful purposes."—13th August 1840.

36. To Downes Edwards of Surbiton Hill, Kingston, in the county of Surrey, farmer, "improvements in preserving potatoes and other vegetable substances."—13th August 1840.

37. To William Crane Wilkins and Matthew Samuel Kendrick of Long Acre, in the county of Middlesex, lamp manufacturers, "certain improvements in lighting and in lamps."—13th August 1840.

38. To Charles Wheatstone of Conduit Street, Hanover Square, in the county of Middlesex, Esq., and William Fothergill Cooke of Copthall Buildings, in the city of London, Esq., "improvements in giving signals and sounding alarums at distant places, by means of electric currents."—21st August 1840.

39. To George Saunders of Hooknorton, in the county of Oxford, clerk, and James Wilmot of the same place, farrier, "improvements in machinery for dibbling or setting wheat and other grain."—25th August 1840.

40. To Charles Wye Williams of Liverpool, in the county of Lancaster, gentleman, "improvements in the means of generating heat principally applicable to the production of steam and the prevention of smoke."—28th August 1840.

41. To Thomas Gadd Matthews of the city of Bristol, merchant, and Robert Leonard of the same place, merchant, "certain improvements in machinery or apparatus for sawing, rasping, or dividing woods or tanners' bark."—31st August 1840.

42. To Miles Berry of the Office of Patents, 66 Chancery Lane, in the county of Middlesex, patent agent, being a communication from abroad, "certain improvements in the strengthening and preserving ligneous and textile substances."—1st September 1840.

43. To Peter Fairbairn of Leeds, in the county of York, engineer, being a communication from abroad, "certain improvements in machinery or apparatus for heckling, combing, preparing, or dressing hemp, flax, and such other textile or fibrous materials."—7th September 1840.

44. To Thomas Milner of Liverpool, in the county of Lancaster, "certain improvements in boxes, safes, or other depositories, for the protection of papers or other materials from fire."—8th September 1840.

45. To John Johnston of Glasgow, in the county of Lanark, North Britain, gentleman, "a new method (by means of machinery) of ascertaining the velocity of, or the space passed through by ships, vessels, carriages, and other means of locomotion, part of which is also applicable to the measurement of time."—14th September 1840.

46. To Edwin Travis of Shaw Mills, near Oldham, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus for preparing cotton and other fibrous materials for spinning."—15th September 1840.

47. To Henry Curzon of the borough of Kidderminster, in the county of Worcester, machinist, "improvements in steam-engines."—16th September 1840.

48. To George Gwynne of Portland Terrace, Regent's Park, in the county of Middlesex, gentleman, "improvements in the manufacture of candles, and operating upon oils and fats."—16th September 1840.

49. To Henry Waterton of Fulmer Place, Gerard's Cross, in the county of Buckingham, Esq., "certain improvements in the manufacture of sal ammoniac."—16th September 1840.

50. To John Gibson and Thomas Muir, both of Glasgow, in the kingdom of Scotland, silk-manufacturers, "improvements in cleaning silk and other fibrous substances."—17th September 1840.

51. To James Stirling of Dundee, engineer, and Robert Stirling, clerk, D. D. of Galston, Ayrshire, "certain improvements in air-engines."—17th September 1840.

52. To James Harvey of Bazing Place, Waterloo Road, in the county of Surrey, gentleman, "improvements in extracting sulphur from pyrites and other substances containing the same."—21st September 1840.

53. To Gerard Ralston of Tokenhouse Yard, in the city of London, merchant, being a communication from abroad, "improvements in rolling puddle-balls or other masses of iron."—22d September 1840.

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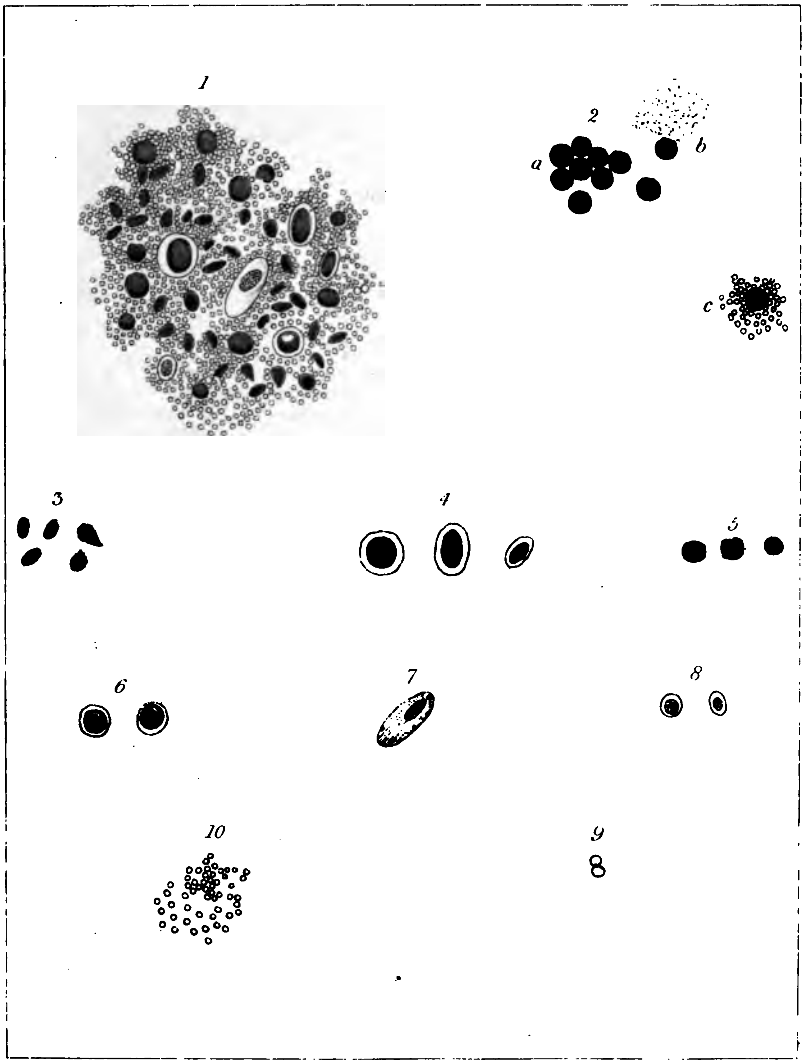

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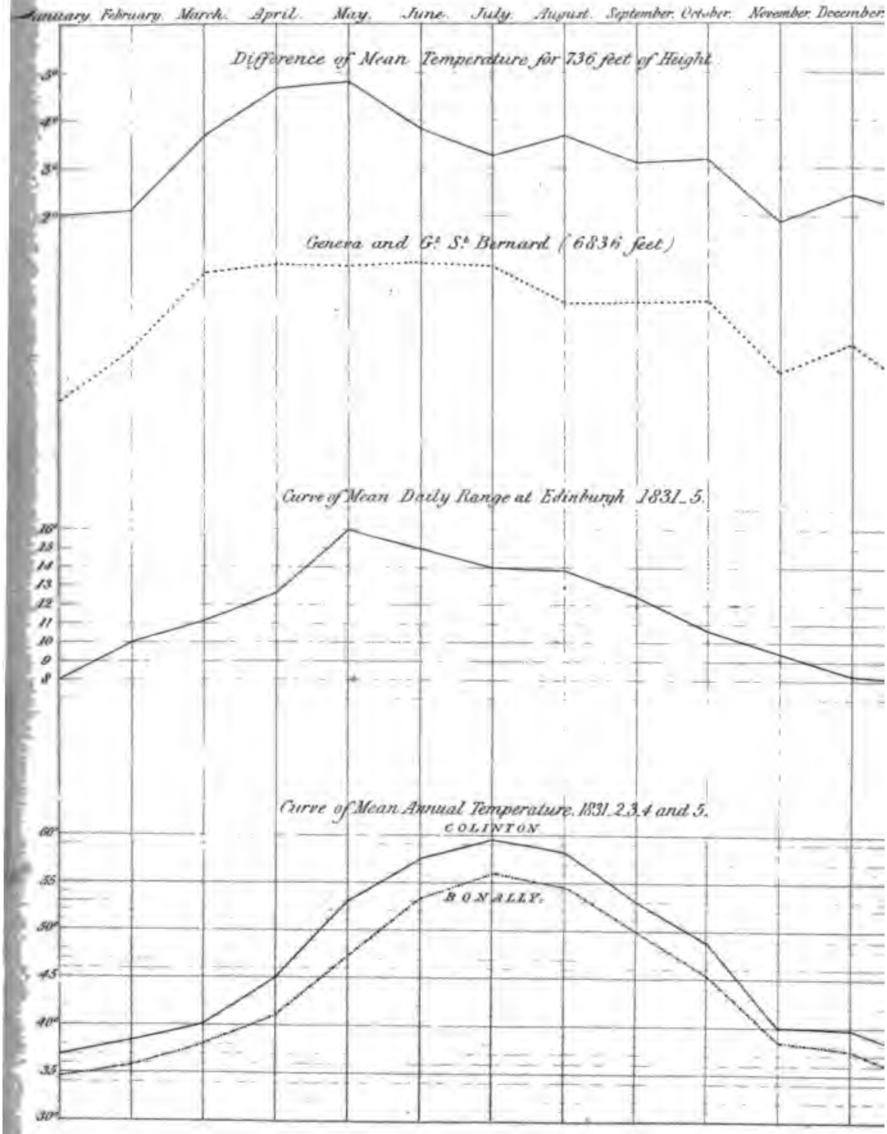
DIMINUTION OF TEMPERATURE WITH HEIGHT.



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